THE

ASTROPHYSICAL JOURNAL

AN INTERNATIONAL REVIEW OF SPECTROSCOPY
AND ASTRONOMICAL PHYSICS

VOLUME XII — OCTOBER, 1900 — NUMBER 3

ON THE ARC-SPECTRA OF SOME METALS, AS INFLUENCED BY AN ATMOSPHERE OF HYDROGEN.

By HENRY CREW.

THE orderly arrangement of lines in the arc-spectra of metals belonging to the first and second groups of Mendelejeff's table and the apparent disorder among the lines of the remaining groups constitute one of the most striking features of spectroscopic science in its present state. That this disorder is only apparent is evident not alone on *a priori* grounds but also from the fact that, among these very elements, many pairs and groups of lines with constant frequency-differences have been discovered.

One great barrier to the discovery of more complete order in these spectra is the enormous number of lines with which the metals of these groups are burdened.

In order to simplify matters the first step would, therefore, appear to be the discovery of some basis upon which the spectral lines of any one element might be separated into smaller divisions. Such an analysis would at least greatly increase the chances of finding new series.

Such a basis of division has already been hinted at by Kayser,[1] who found that nearly two thirds of all the reversed lines of palladium were arranged in triplets having the same frequency-differences. In short, reversibility appears to be a distinguishing feature of a group of palladium lines whose law of distribution is yet unknown.

It occurred to the writer that possibly another basis of division might be found in the effects produced by surrounding the arc with various atmospheres of gas, as, for instance, hydrogen, nitrogen, coal gas, ammonia.

The experiment[2] with hydrogen was tried as follows: An arc was operated between two metallic electrodes, one of which was in rapid rotation while the other was fed in slowly by means of a screw.

This arc, including the rotating electrode, was placed in a brass hood which was made of two parts, not unlike the Magdeburg hemispheres, except that these two hemispheres, instead of having flat faces, were threaded with a screw and provided with accurately turned flanges, so that, when closed, the hood was gas-tight.

Into one of these hemispheres were fitted the two electrodes, one by means of a gas-tight bearing, the other by means of a gas-tight nut-and-screw. It was necessary, of course, to insulate at least one of these electrodes from the brass hood: this was done by placing the nut-and-screw on a slate plate, which in turn was screwed to the brass hood. The shaft which carried the rotating electrode and the current could, therefore, touch the brass hood with impunity; but, as a matter of fact, this was not the case. On the contrary, the shaft fitted loosely into a bearing which was packed with clean asbestos. This packing served not only the purpose of insulation but also to prevent any oil reaching the interior of the hood. Consequently

[1] KAYSER: "Bogen-spectra der Elementen der Platin-gruppe." *Abh. Berl. Akad.*, 1897, p. 42.

[2] The entire apparatus described below and the experiments depending upon it were made possible only through the generous consideration of the committee of the American Academy of Arts and Sciences in charge of the Rumford Fund.

no hydrocarbons were formed at the arc unless from carbon impurities in the electrodes themselves.

On the side opposite the arc, the hood was provided with an opening into which screwed a brass tube about one foot long. At its outer end, this tube carried an image-lens of quartz, which projected the arc upon the slit of a 10-foot concave Rowland spectrograph.

The hydrogen was furnished by three large electrolytic cells, made with sheet-lead electrodes in dilute sulphuric acid. Each cell absorbed twelve amperes of current, so that the hydrogen produced by thirty-six amperes was continually flowing through the hood. This hydrogen, which was introduced through a stop-cock on one side, was allowed to escape through a stop-cock on the opposite side of the hood, where it burned continuously as a pilot flame about two centimeters high. In the current of hydrogen a trap, instead of a drying tube, was used, for it was found by experiment that there was no difference in effect between hydrogen which had been dried by phosphorus pentoxide and sulphuric acid, and hydrogen which had not been dried at all.

The most marked effect of hydrogen on the arc-spectrum is a general diminution of intensity. Consequently, in order to photograph the spectrum of the arc *in hydrogen* so as to give, on the negative, an average intensity equal to that of the arc *in air*, one must make the exposure in hydrogen from 5 to 100 times as long as in air.

The most interesting effect of the hydrogen atmosphere, however, is not the change in the average intensity of the entire spectrum, but the change of relative intensity among the lines of any one substance. This change was studied by photographing on each negative three spectra side by side. The first was that of the arc in air, the second that of the arc in hydrogen, the third that of the arc in air. But the exposure was always so timed that the spectrum in hydrogen had an intensity which was intermediate between the two intensities in air. The advantage of this is that, in comparing intensities, if a line is apparently

weakened one has a still weaker spectrum with which to compare it, and hence can decide whether the weakening is due to under-exposure or due to some effect on this particular line and not on the whole spectrum. In like manner, if a line is apparently intensified, one has a still stronger spectrum with which to compare it, and to convince himself that the intensification is not due to over-exposure.

The effects of hydrogen on the spectra of magnesium and zinc are partially summarized in Tables I and II, which follow.

TABLE I.

ARC-SPECTRUM OF MAGNESIUM AS MODIFIED BY AN ATMOSPHERE OF HYDROGEN.

Wave-length of lines which are relatively			Remarks
intensified	weakened	unaffected	
		5711.31	
		5528.64	
		5183.79	Second subordinate series: Kayser and Runge.
		5172.86	
		5167.50	
	[5007.47]		First head of magnesium-oxide fluting. This and the six following bands are, of course, completely blotted out.
		4730.42	Line too weak to compare.
		4703.18	Widened towards red.
	4571.28		Intensity halved.
4481.			Intensity increased perhaps ten times: line enormously widened. This line does not appear in the ordinary carbon-magnesium arc.
		4352.08	Widened towards red.
	[4216.12]		Cyanogen, Band II, which appears as an impurity: is completely blotted out, owing to absence of nitrogen.
		4167.81	
		4058.45	
		3987.08	
	[3883.55]		Cyanogen, Band III: impurity: completely blotted out.
		3838.44	First subordinate series: Kayser and Runge.
		3832.45	
		3829.50	
	[3590.48]		Cyanogen, Band IV: does not quite disappear.
		3336.82	Second subordinate series.
		3332.33	
		3330.04	

TABLE I.—*Continued.*

Wave-length of lines which are relatively			Remarks
intensified	weakened	unaffected	
		3097.01 **3093.09** **3091.18**	First subordinate series.
		2942.21 **2938.67** **2936.99**	Second subordinate series.
2936.61 2928.74			Strong spark lines.
		2915.57	
	2852.22		Strong spark line: width of reversed portion increased at least ten times: line, as a whole, nearly extinguished.
		[**2852.22**] **2848.53** **2846.91**	First subordinate series. From analogy, the first line of this triplet, here covered by the heavy spark line, is probably unaffected by hydrogen.
		2802.80	Reversal greatly increased.
		2798.07	
		2795.63	Reversal greatly increased. A second reversal, not occurring in the ordinary arc, appears in the shade of this line at 0.7 tenth-meters towards the violet.
		2790.88	
		2783.08	
		2781.52	
		2779.94	Width and reversal increased.
		2778.38	
		2776.80	
	2768.57		

The line at 2765.47 and the five triplets of still shorter wave-length which complete Kayser and Runge's list of magnesium lines are so completely blotted out by the hydrogen atmosphere that no comparison in this region is possible — even on plates of four hours exposure. And this is true while many sharp lines, impurities, of shorter wave-length yet appear on the negative.

TABLE II.

SPECTRUM OF ZINC ARC AS MODIFIED BY AN ATMOSPHERE OF HYDROGEN.

Wave-lengths of lines which are relatively			Remarks
intensified	weakened	unaffected	
5182.20			
		4810.71	Second subordinate series.
		4722.26	
		4680.38	
	4630.06		Shading towards red greatly increased.
	4298.54		Too faint to compare with certainty; apparently weakened.
	4293.02		
	4101.94		
	[3683.63]		Two very persistent lead lines which, as impurities, are greatly weakened by hydrogen.
	[3679.72]		
		3315.26	Not strong enough for comparison.
		3346.04	
		3345.62	First subordinate series.
		3345.13	
		3303.03	
		3302.67	
		3282.42	
	3075.99		
		3072.19	Second subordinate series.
		3035.93	
		3018.50	
		2801.00	
	2781.33		
		[2771.05]	Not found.
		2770.94	
		2756.53	
		[2751.49]	No trace of these lines in hydrogen.
		[2736.96]	
		2712.60	Second subordinate series.
		2684.29	
		2670.67	
		2623.87	Invisible in hydrogen.
		2608.65	Last triplet visible on hydrogen negative after two hours exposure.
		2582.57	
		2570.00	
2558.03			Strong spark lines: shortest wave-length visible on hydrogen negative after two hours exposure.
2502.11			

The lines 2601.03, 2575.15, 2562.70, 2138.03, and the remaining six triplets of Kayser and Runge, could not be obtained in the hydrogen atmosphere under any reasonable exposure.

THE IRON SPECTRUM.

In the case of iron, the number of spectral lines is so enormous that we can here merely illustrate the effects of a hydrogen atmosphere by giving the principal changes which occur in the region covered by a single negative.

In the first column of the following table is given the wave-length of the line. These wave-lengths are reliable to about 0.05 of an Ångström unit. The second column gives the intensity of the line in the atmosphere of hydrogen. The scale of intensities runs from "1" for lines just easily visible, to "10" for the heavy lines. In the third column is given the effect produced by hydrogen. Here the word "*new*" is used to indicate that the line does not appear in the ordinary iron arc. "*Enh*" is a contraction for the word "enhanced," and the number which follows "*Enh*" indicates, roughly of course, how many times greater the intensity is in hydrogen than in air. The term "*Dim*" is a contraction of "diminished," and the number following indicates how many times weaker the line is in hydrogen than in air.

The illustrations given are sufficient to show that the hydrogen atmosphere does more than slightly alter these spectra: it profoundly modifies them.

The explanation of these changes is probably simple, but it is not patent. A number of hypotheses thrust themselves upon one's attention, but as a rule they are difficult to test by experiment, and their value is, therefore, not great. For instance, it does not appear impossible that the resistance of the arc may vary considerably with the nature of the atmosphere surrounding it; and, if so, then the temperature and the spectrum may vary. But whether, in such case, the hydrogen would serve to increase or diminish the temperature it is not easy to predict.

In nearly all the spectra which I have photographed an average direct current of about 2 amperes has been employed, while the pressure between the electrodes has varied from 75 to 100 volts. The current was constantly varying in intensity and was often completely interrupted. Is it not possible that an

TABLE III.

ILLUSTRATION FROM SPECTRUM OF IRON ARC IN HYDROGEN.

Wave-length	Intensity	Description	Wave-length	Intensity	Description
4056.13	8	New	3666.95	7	Enh 8
4039.03	7	New	3660.76	2	New
4027.27	8	New	3659.09	7	Enh 20
4000.12	6	New	3652.50	3	Enh 10
3997.16	10	Enh 10	3652.22	3	Enh 10
3944.54	7	Enh 10	3648.52	3	Enh 8
3941.02	1	Dim 7	3645.24	6	Enh 10
3938.37	4	New	4643.78	2	Dim 2
3928.74	4	Enh 6	3642.67	4	New
3926.57	8	Enh 4	3630.46	1	Dim 2
3920.91	10	Enh 7	3626.90	3	New
3917.97	10	Enh 8	3625.27	2	Dim 2
3910.79	10	Enh 10	3620.07	8	Enh 10
3906.61	2	Dim 5	3616.71	7	Enh 5
3899.85	3	Dim 4	3615.88	7	Enh 5
3899.19	12	Enh 6	3612.20	1	Dim 4
3898.09	5	Dim 2	3607.44	7	Enh 10
3897.61	8	Enh 3	3602.60	2	Dim 2
3889.31	5	New	3598.94	8	Enh 8
3866.97	5	Enh 10	3594.68	3	Dim 3
3864.28	5	Enh 5	3593.55	6	Enh 3
3861.66	5	New	3583.92	6	Enh 10
3850.48	4	New	3573.15	5	Enh 5
3843.93	8	Enh 6	3572.06	2	Dim 2
3825.13	4	Enh 10	3540.53	3	Enh 10
3814.63	1	Dim 4	3538.76	5	Enh 10
3807.64	2	Dim 5	3507.15	5	Enh 10
3801.79	8	Enh 4	3499.34	4	Enh 10
3800.57	8	New	3493.60	4	New
3790.23	3	Dim 2	3491.22	3	Enh 4
3786.82	1	Dim 4	3474.19	2	Enh 4
3781.79	3	New	3465.98	4	Dim 2
3775.81	3	New	3460.43	2	Enh 4
3771.71	5	New	3323.20	2	New
3767.78	6	Enh 10	3281.40	2	New
3759.28	3	Enh 8	3277.55	3	Enh 5
3739.98	10	Enh 10	3274.09	1	Dim 5
3724.09	4	Enh 8	3265.17	0	Dim 6
3723.30	4	Enh 8	3264.64	0	Dim 4
3718.04	3	New	3259.17	6	New
3706.16	3	Enh 10	3258.89	4	New
3700.36	4	New	3257.70	2	Dim 2
3692.92	5	New	3251.36	0	Dim 5
3692.08	5	Enh 12	3247.67	1	Dim 6
3688.29	6	Enh 10	3246.13	0	Dim 10
3683.19	3	Dim 3	3243.87	2	Enh 4
3680.08	4	Dim 2	3237.95	2	New
3671.78	2	Enh 4	3236.35	1	Dim 8
3668.50	6	Enh 7	3234.75	0	Dim 10

atmosphere of hydrogen increases the rapidity of these interruptions, and hence alters the effects of self-induction, thereby changing the temperature of the arc?

Still a third hypothesis is that the introduction of hydrogen prevents the formation of oxygen and nitrogen compounds, thus blotting out some lines, and that it permits the formation of hydrogen compounds, thus accounting for the appearance of new lines.

Whatever the explanation may be, the following curious relation was found to exist between the arc- and spark-spectra, namely, *all lines in the arc-spectrum which are affected by hydrogen, whether enhanced or diminished, belong to the spark-spectrum also.*

To illustrate: the arc-spectrum of tin in hydrogen shows two strong lines at λ 3362.15 and λ 3283.31, of which there is not the faintest trace in the ordinary tin arc. But these two are among the strong lines of the tin spark. See Hartley and Adeney's list. In like manner I have photographed on the same plate the iron spark in air, the iron arc in air, and the iron arc in hydrogen. In every case examined, the lines affected by hydrogen are spark lines.

On the other hand, *the lines which belong to Kayser and Runge's series are unaffected by the change from air to hydrogen.* If these series prove equally stable in other gases, this stability may form a criterion for dividing a spectrum into two groups, one of which will contain all the series lines, the other of which will contain none of them.

NORTHWESTERN UNIVERSITY,
Evanston, Ill.

SOME ABNORMAL STARS IN THE CLUSTER *M 13 HERCULIS.*

By E. E. Barnard.

For a long time I have been engaged in a micrometrical determination of the positions of a number of the individual stars in the great globular clusters *M 3*, *M 5*, *M 13*, *M 15*, and *M 92*.

In this work several peculiarities have come up that have seemed of importance. The most striking of these is the fact that some of the stars in these clusters shine with a much bluer light than the great majority. This produces a most remarkable difference between their photographic and visual images. In comparing photographs with the actual sky one is often struck with the relative smallness of certain stars on the photographs that are bright in the sky. This peculiarity is due to more or less color in these stars which produces a smaller photographic image. This is sometimes so pronounced that it is troublesome to identify the individual stars.

What one finds in the clusters is just the reverse of this, *i. e.*, certain of the stars appear very much larger on the photographs than they do in the sky. So striking is this in some cases as to at first suggest variability.

This peculiarity was noticed among the first observations of the clusters made here, and attention was called to it at the meetings of astronomers at Harvard College Observatory in 1898, and at the Yerkes Observatory in 1899.

The comparisons were made with a negative enlarged about four times from the original photograph of *M 13 Herculis*, taken with the Potsdam 13-inch photographic refractor in 1891, which was measured by Professor Scheiner for his catalogue of 833 stars of this cluster. This photograph was kindly loaned me by Professor Frost, who assisted Professor Scheiner in his work on the cluster.

The most striking of these photographically bright stars in *M 13* are Nos. 148, 179, 382, 393, and 749 of Scheiner's catalogue. The brightest of these is No. 148, which is also perhaps the most striking example. Since this star is an outlier, it is conspicuous on all photographs of the cluster. On the Potsdam and other photographs it is the brightest star belonging to the cluster. It several times outranks all the stars near it in brightness, and stands out conspicuously above all its neighbors. North preceding it about 19″, is the star No. 131; on the photograph this last star appears very small and inconspicuous, compared with 148. In the sky, however, they are almost identical in brightness, though on the photograph 148 is four or five times larger. Some 68″ following, and south, is No. 269, which on the photograph appears four or five times smaller than 148, but somewhat larger than 131. In the telescope No. 296 is nearly one magnitude brighter than 148. Yet 131 and 296 and their surrounding stars bear the same light ratios in the sky that they do on the photograph, No. 148 being alone photographically abnormally bright. This star was tested with a high magnifying power to see if it came to a focus different from the stars near it, but there did not seem to be any difference in this particular. It was seen, however, that the image of 148 did not, under the best conditions, appear as sharp as the stars near it; it gave the impression of a slight fuzziness; at such times it has suggested the idea of a minute nebula, or rather of a minute planetary nebula. The magnitude of this star as given by Scheiner is 11.7, making it the brightest star with one exception (that of a more distant outlier, also 11.7) of any in the cluster. The two other stars, 131 and 296, are rated respectively as $12^{m}.7$ and $12^{m}.4$.

No. 382 is Scheiner's normal star, and is assigned magnitude 12.7; visually it cannot exceed $14^{m}.5$ and is very much fainter than other stars which match it on the photograph.

No. 749 is visually exactly equal to No. 763, but on the Potsdam photograph it is three or four times larger than the latter star.

No. 393 is very difficult to see at all, though it is assigned magnitude 12.7 and on the photograph appears as large as other stars near it which immensely outshine it in the telescope. It is the most striking example of faintness, visually, of these stars. Yet the majority of the stars on the photographs bear the same relative size to each other that they do in the sky. The phenomenon is therefore an exceptional feature, pointing to something remarkable in these particular stars not possessed by the great majority of stars in the cluster.

No. 179 is a striking case because of its close proximity to a star of normal condition, No. 183. On the photograph they are almost exactly equal, while in the sky 179 is many times smaller (about $14^{m}.5$ to 15^{m}) and very faint. There are really two small stars at this place, close together, the preceding one of which is slightly the brighter. Of these two small stars the photograph shows that the peculiarity lies only in the preceding star. The position of the brighter one with reference to 183 is

$$207^{\circ}.0 \quad 7''.78.$$

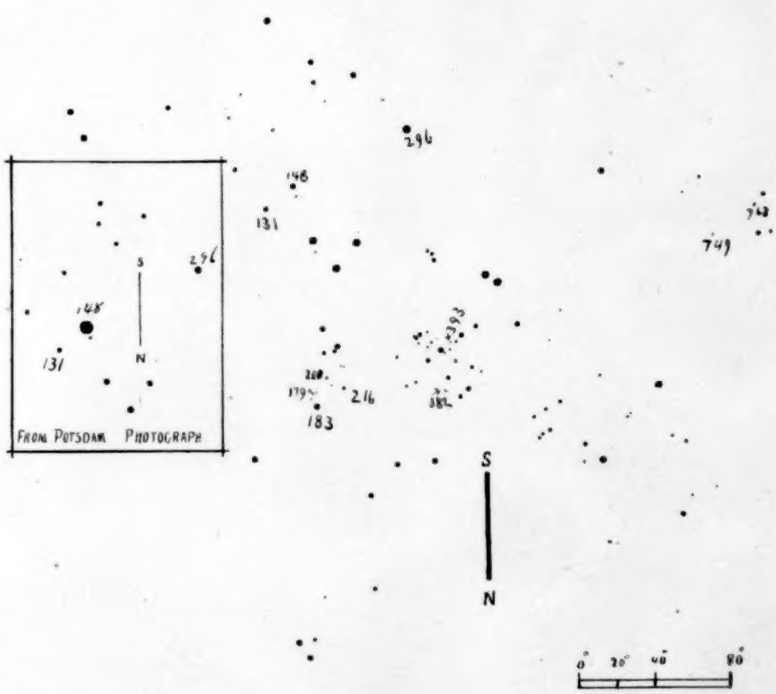
The star 216, which is shown to be variable with the 40-inch, seems also to be abnormal—because on the Potsdam photograph it appears considerably brighter than 183, which it never equals visually.

The accompanying diagram will show the location of the stars referred to. The small diagram from the Potsdam photograph shows the relative size of the abnormal star 148 compared with the stars near it, while the larger diagram gives its appearance as seen in the telescope. The star 393 is in the heart of the cluster. It will be easy to identify any of these stars on photographs of the cluster that show them. They are platted from the micrometer measures.

Of course the simple explanation of this peculiarity is that these stars, so bright photographically and so faint visually, are shining with a much bluer light than the stars which make up the main body of the cluster. This is an interesting fact, and would seem to be an important one.

This peculiarity does not exist alone in *M 13*, for I have found similar cases in *M 5 Librae*, and it doubtless exists in the other clusters.

Speaking with Professor Hale a short time ago on the subject, he suggested that a photograph made with a yellow color-



screen ought not to show this peculiarity, and that the stars on such a photograph should be closely comparable with the visual appearance. A photograph of *M 13* made the next night by Mr. Ritchey with a yellow color-screen on the 40-inch showed that this statement was correct, for on this picture 148, 382, 179, etc., appeared with the same relative brightness that they have in the telescope.

It would be an interesting thing to know just what these abnormal stars are. At present the spectroscope is unable to deal

with such faint objects. The fact remains, however, that we have in the globular clusters, relatively, a few stars which differ widely from their fellow stars in the same cluster, and which seem to shine with a strong blue light, resembling in this respect the nucleus of the annular nebula of *Lyra* (*M 57*) and perhaps bearing the same relation to the ordinary stars of the cluster that the nucleus of that nebula bears to the ordinary stars of the sky.

As bearing on the possible existence of minute nebulæ in the clusters, I have found a very small nebula preceding *M 15* which, if much smaller, would be taken for an ordinary star. It is 13th magnitude and by micrometer measures is $2''.1$ in extreme diameter. It appears as a very small hazy star which, with the higher powers, is seen to be an extremely small nebula. It precedes the center of the cluster by $1^m\ 7^s$ and is $1\frac{1}{2}'$ north. Its position for 1860.0 is therefore $\alpha = 21^h\ 22^m\ 6^s$, $\delta = +11°\ 34'.8$.

Position of the nebula with reference to $13^m.5$ star (s. of 2) 1899 Aug. 15: P. A. $220°.4$, Dist. $28''.48$. The nebula and the two small stars lie midway between two $12^m.8$ stars which are $5'$ apart. The double star whose measures are given below follows the nebula by $13^s \pm$ and is about $1\frac{1}{4}'$ north.

Closely preceding the cluster is what seems to be another very minute nebula, but I could never be certain that it is a nebula—it is so small. It is 14^m and precedes the center of *M 15* by $5'$ and is $1'$ north.

Small double star $0^m\ 52^s$ preceding and $2'$ north of the center of *M 15:*

(Approximate position 1900.0: $\alpha = 21^h\ 22^m\ 21^s$, $\delta = +11°\ 34'.9$)

1898.610	Aug. 10	$62°.5$	$0''.97$		
1899.623	Aug. 15	60.7	0.92	$12^m.5$	$13^m.5$
1899.738	Sept. 26	61.7	1.08		
1899.32		$61°.6$	$0''.99$		

This is a new double star.

The possibility of these abnormal stars being of the nature of nebulæ brings up the question of nebulosity in the globular clusters. In the observations here, sometimes under very fine

conditions, I have become convinced that the great clusters are not nebulous, as has been claimed from insufficient evidence, and this confirms my observations made with the 36-inch at the Lick Observatory.[1] Under the best conditions there is no appearance of true nebulosity. Professor Keeler has lately shown from photographs with the Crossley reflector that there is no evidence of nebulosity even from a photographic standpoint.

YERKES OBSERVATORY,
August 1900.

[1] See *Monthly Notices*, November 1896, **57**, 11-12.

DISCOVERY AND PERIOD OF A SMALL VARIABLE STAR IN THE CLUSTER *M 13 HERCULIS.*

By E. E. Barnard.

While engaged in micrometrical work on the stars in the great globular clusters, I have become deeply interested in the variable stars found by Professor Bailey on his photographs of these objects taken at Arequipa, and a large number of observations of some of these stars has been secured and will be printed with the micrometrical work when it is issued.

The great cluster of *Hercules*, *M 13*, at first seemed to differ from the other globular clusters in having no variable stars within its boundaries. This seemed to be the case as late as 1898. About that time, however, Professor Bailey found two stars slightly variable among the outliers on the Harvard photographs. I have simply heard that such were found, but have seen nothing stated as to their position in the cluster so that they might be identified and observed. I therefore do not know of the location of any such variables, and have made no special search for such stars. During the micrometer measures, however, I have found a variable star just within the edge of the brightest part of the cluster.

This star was found entirely through the visual observations and not from an examination of photographs. Indeed, the only photograph that I had seen up to the time of finding it that showed the star at all was the Potsdam photograph. Its region is burned out in the other pictures by over-exposure. I have identified it as No. 216 of Scheiner's catalogue of stars measured on a photograph of *M 13*. Its position has been micrometrically measured with reference to other stars of the cluster.

Scheiner Nos. 205 and 216,

$14^{\circ}.12$ (2) $21'.49$ (2).

Scheiner Nos. 183 and 216,

$129^{\circ}.53$ (4) $17'.50$ (4).

Scheiner Nos. 373 and 216,

$288^{\circ}.45$ (2) $55'.16$ (2).

The variation is about one entire magnitude, from the 13th to the 14th, or rather from the 14th to the 13th, because its normal condition is faint.

From observations covering an interval of 70 periods, from 1899, August 14, to 1900, August 7, I deduce a period of 5 days, 2 hours, and 24 minutes ($5^{d}.10$). From approximate elements its light curve seems to be much like the ones found by Professor Bailey for the variables in *M 5 Librae.* The rise in brightness is rapid and the decline relatively slow. The star takes about 1 day to rise to its maximum, and its decline occupies about 2½ or 3 days. The rise is therefore about 0.2 and the decline about 0.5 or 0.6 of the entire period.

The photographic magnitude given this object star by Professor Scheiner is 12.4. I have compared the light of the variable with No. 200 near and preceding it.

Following are the observations:

COMPARISON OF THE LIGHT OF THE VARIABLE (NO. 216) WITH NO. 200.

(Time 6 hours slow of Greenwich.)

1900, July 9	$0^{m}.2$	brighter than No. 200 at	$11^{h}\ 35^{m}$.
10	0 .1		11 10
10	0 .4		14 0
11	1 .0		9 40
12	0 .7		10 10
24	0 .1		10 0
24	0 .1		12 25
25	0 .1		9 15
25	0 .0		10 20
26	0 .7		12 0

1900, July 28	$0^m.3$ brighter than No. 200 at	9^h	15^m.
29	0 .1	9	10
29	0 .1	10	55
30	0 .0	9	45
30	0 .1	12	0
31	0 .7	8	50
31	0 .7	10	10
Aug. 1	1 .0	10	10
4	0 .1	8	15
5	0 .7	12	0
6	1 .0	8	35
7	0 .5	8	15
12	0 .5	8	35
13	0 .1	8	50
14	0 .0	8	5
18	0 .1	8	25
20	0 .1	8	00
21	1 .0	8	40
27	0 .6	7	35
28	0 .3	9	00
Sept. 3	0 .1	7	20
3	0 .2	7	50
4	0 .1	7	15
4	0 .2	8	0
19	0 .2	10	10
Oct. 2	1 .0	6	40

This star was also at its maximum on August 14, 1899. For purpose of identification, it is marked on the accompanying chart.

YERKES OBSERVATORY,
August 1900.

SOLAR PHENOMENA, CONSIDERED IN CONNECTION WITH ANOMALOUS DISPERSION OF LIGHT.[1]

By W. H. JULIUS.

THE rule that the propagation of light is, in all directions, rectilinear, holds only for quite homogeneous media. If various considerations lead us to assume that the solar rays on their course penetrate media of unequal density, or of different composition, the rays must be curved, and the supposition that the observed light is emitted by objects situated in the direction of vision becomes untenable.

Now, though no one doubts the unequal distribution of matter in and near the Sun, yet in theories concerning this celestial body hardly any attention has been paid to refraction. The study of atmospheric refraction had, long since, made us acquainted with the laws of curved rays[2] but the first important attempt to investigate the influence which refraction in the Sun itself must have had on the course of the rays which reach our eye, and consequently on the optical image we get of it, was made by Dr. A. Schmidt. His paper "Die Strahlenbrechung auf der Sonne; ein geometrischer Beitrag zur Sonnenphysik"[3] leads to very remarkable results, and at any rate urges the necessity of submitting the existing theories of the Sun to a severe criticism from this point of view.

If it is taken for granted that refraction in the solar atmosphere must be taken into account we must also pay attention to those special cases in which extraordinary values — great or small — of the refractive index occur; in other words, the phenomenon of anomalous dispersion must be reckoned with.

[1] *Proceedings of the Royal Academy of Sciences*, Amsterdam.

[2] The literature on this subject may be found in a dissertation by O. Wiener, *Wied. Ann.*, 49, 105-149, 1893.

[3] Stuttgart, Verlag von J. B. Metzler, 1891.

It is my purpose to show that many peculiarities, which have been observed at the limb of the Sun and in the spots, may easily be considered as caused by anomalous dispersion.

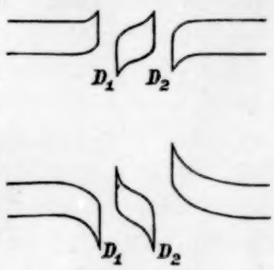
It is not difficult to obtain the experimental evidence that the index of refraction of sodium vapor for light differing but slightly in wave-length from that for the D lines, is very different from the index for the other rays of the spectrum.

H. Becquerel[1] used for the study of the phenomenon Kundt's method of crossed prisms, in a slightly modified manner. The image of the crater of an arc light was projected on a horizontal slit, placed in the focus of a collimator-lens. The parallel beam next passed through a sodium flame, which Becquerel had succeeded in giving the form of a prism with horizontal refracting edge, and was then, through a telescope lens, focused into an image of the horizontal slit, falling exactly on the vertical slit of a spectroscope of rather great dispersion. As long as the sodium flame was absent, a continuous spectrum was seen in the spectroscope, the height of which naturally depended on the width of the horizontal slit. When the flame was introduced in its proper place, and good care was taken to limit the parallel beam by means of an easily adjusted diaphragm, in such a manner that only such light could penetrate into the telescope lens as had passed a properly prismatical part of the flame, the spectrum clearly exhibited the anomalous dispersion. On either side of the two dark sodium lines the originally horizontal spectrum-band was boldly curved, so that for rays with wave-lengths slightly larger than λ_{D_1} or λ_{D_2}, the sodium vapor appeared to possess an index of refraction rapidly increasing in the neighborhood of an absorption line; whereas for rays of wave-lengths slightly smaller than λ_{D_1} or λ_{D_2}, the index of refraction rapidly decreased when approaching the absorption lines. The amount of the anomalous dispersion near D_2 exceeded that near D_1.

In repeating this experiment I obtained materially the same results. Moreover, I noticed a peculiarity in the phenomenon, not mentioned by Becquerel, and not exhibited in the diagrams

[1] *C. R.*, **127**, 399; and **128**, 145.

accompanying his paper. Becquerel states that when he introduced a flame, rich in sodium, the lines D_1 and D_2 appeared as broad, dark bands, and that on either side of both bands the spectrum was curved. According to his diagrams these displacements only refer to light outside the bands; the rays inside this region, in the more immediate neighborhood of the D lines, are totally wanting. Fig. 1 refers to a prismatic part of the flame, edge upwards; Fig. 2, to a prismatic part, edge downwards. Both cases represent the image as seen in a telescope, and are thus reversed.



FIGS. 1 AND 2.

I myself, however, have observed the phenomenon in the form of Fig. 3. The dotted lines indicate the places of D_1 and D_2. When the electric light is intercepted by means of a screen introduced between the flame and the horizontal slit, the D lines appear in those places as two faintly luminous, sharply defined slit-images. The light is faint because the flame is placed at a distance of more than 70 cm from the vertical slit, and its radiation is all but intercepted by the adjustable diaphragm, which allows only a beam of a cross-section of about 0.2 cm^2, to enter the telescope lens.

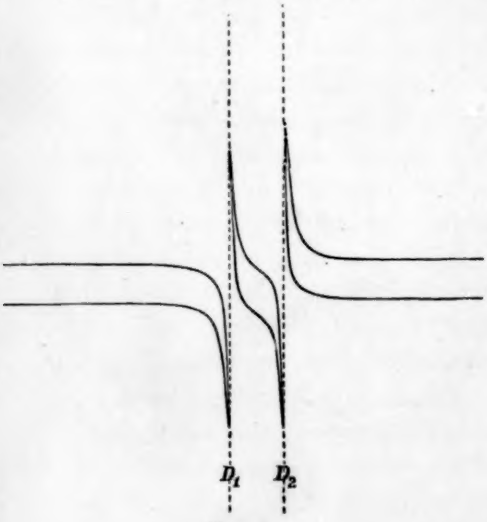


FIG. 3.

When next the arc light is allowed to cross the flame, the spectrum of Fig. 3 appears with such intensity that the bright sodium lines are undistinguishable in the center of the dark bands. In the upper and lower parts of the field of vision, however, they can yet be seen as continuations of the

four bright arrows of light which are, as it were, flashed forth from the horizontal spectrum into the dark.

By repeatedly intercepting and readmitting the light of the main source, I have actually convinced myself that the intense arrow-light, with the dispersion used, gradually passes into the faint light of the emission-lines, both with respect to intensity and place in the spectrum. A flat Rowland grating with 47,000 lines was used in the spectroscope; one spectrum of the first order being extremely brilliant. The crosswires of a micrometer eyepiece (65 divisions of which correspond to the distance of the D lines in the first diffraction spectrum) were repeatedly adjusted as close as possible to the extreme part that was yet distinctly visible of such an arrow, the sodium lines of the flame being all but invisible. I next removed the diaphragm near the flame, intercepted the main light so that the sodium lines now became clearly visible, and took a number of the readings of the emission line. The mean readings of two series of observations did not mutually differ by one division; the arrow, therefore, approached the D line to within 0.01 $\mu\mu$. From the data furnished by Becquerel[1] it can be inferred that the distance between the D lines and the most deflected parts of the arrows upon which, in his experiments, the crosswires could still be adjusted, was on an average greater than 0.1 $\mu\mu$.

I am not quite sure how this difference in the results must be accounted for; perhaps Becquerel's flame contained more sodium than mine; anyhow so much sodium is not wanted to produce strong anomalous dispersion.

The following experiment convinced me how narrow was in reality the absorption-region of each of the sodium lines. An additional lens of 20 cm focal distance was placed between the telescope lens and the vertical slit, in such a manner that on this slit was thrown the image of the prismatic part of the sodium flame, and not that of the horizontal slit, as before. In this image, therefore, all rays that had passed the flame and had been refracted in different directions, must be found reunited.

[1] *C. R.*, **128**, 146.

The absorption lines were now actually very narrow, the emission lines in some places all but covering them.

The additional lens being removed, the light-arrows forthwith reappeared above and below the rather broad dark bands in the curved spectrum.

It appears, therefore, from our observations that in spite of the considerable width of the dark bands in the main spectrum, the corresponding light is but very slightly absorbed by the sodium lines. The flame has allowed every kind of light to pass, even that of which the wave-length differed ever so little from that of the D lines; but it has caused these rays to be deflected from the straight line much more forcibly than the other parts of the spectrum lying further removed from the absorption lines.

Here, then, we have a case where the absorption spectrum of a vapor exhibits broad bands not deserving the name of absorption bands. The special manner in which the experiment was made enabled us to see what had become of the light that had disappeared around the sodium lines; but very likely the broad bands would have been attributed entirely to absorption if somehow this abnormally refracted light had fallen outside the field of vision of the spectroscope. In studying the absorption spectra of gases and vapors, we should be careful to see—which is not always done—that the absorbing layer shall have equal density in all its parts and shall not act anywhere as a prism. It would be worth while investigating in how far the anomalous dispersion can have influenced cases in which broadening or reversal of absorption lines have been observed.

In my arrangement the absorption lines were narrow, if the main light had passed through a nearly homogeneous and *non*-prismatic part of the flame.

The experiment, as described above, offers no opportunity for obtaining reliable values of the refractive indices. A better method to arrive at more reliable results is now being investigated; for the present all we can say is that the deviation of rays whose wave-length is very near λ_{D_1}, or λ_{D_2}, is at least

six or eight times greater than that which the remoter parts of the spectrum were subject to. Becquerel says that the index for waves greater than λ_{D_1} and λ_{D_2} may attain 1.0009; for waves on the other side of the absorption line the index falls considerably below unity. The line D_2 produces in a much higher degree than D_1 refractive indices smaller than unity;[1] the very high indices are represented in pretty much the same degree near D_1 and D_2.

From all this we infer:

1. Where light emitted by a source that yields a continuous spectrum traverses a space in which sodium vapor is unequally distributed, the rays in the neighborhood of the D lines will be much further deflected from their course than any others. Of all things this holds good of those rays whose wave-length differs so little from λ_{D_1} and λ_{D_2} that they can hardly be distinguished from sodium light. A pretty strong light, therefore, misleadingly resembling sodium light, but in reality owing its existence to other sources, may seem to proceed from a faintly luminous sodium vapor, in a direction deviating from that of the incident light.

2. A spectroscopic examination of the light that has traversed, in a fairly rectilinear direction, the space filled with sodium vapor, shows, in the places where the D lines are to be found, broad dark bands, owing to the fact that the light of these places in the spectrum has deviated sideways from its course and has not reached the slit of the spectroscope.

The former of these inferences we will now apply to certain phenomena in the neighborhood of the disk of the Sun; the latter to some peculiarities of the Sun-spots.

Let the arc ZZ' (Fig. 4) represent a part of the disk of the Sun, the observer being stationed far off in the direction of O. This ZZ' may be taken to be either the limit of the photosphere, or the critical sphere which, in A. Schmidt's theory of the Sun, plays such an important part. In either case, a ray emitted from

[1] In Fig. 3, page 187, the upper arrow near D_2 is defective and rather short compared with that near D_1.

any point A on the surface, at an angle of nearly 90°, will reach the point O along a path the curvature of which diminishes regularly, if we assume that the density of the Sun's atmosphere in a direction normal to the surface decreases continuously.

A ray emitted from B under the same circumstances will proceed along BO' and does not, therefore, reach O; the observer at O will see A lying just within the margin of the disk of the Sun; light proceeding from B is invisible to his eye. Slight irregularities of density in the atmosphere on the path AO will

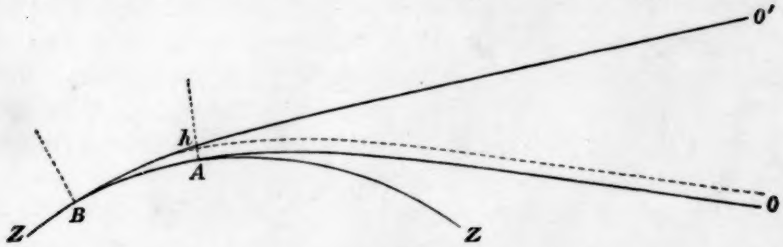


FIG. 4.

indeed deflect the course of the rays, but only slightly if the gases have a normal index of refraction. The irregularities show themselves as shallow elevations and depressions in the edge of the Sun's disk. In the same manner, rays like BO' do not materially deviate from the course which they would have to follow in a perfectly calm atmosphere of continuously decreasing density.

Let us now suppose unequally distributed sodium vapor to be present near A above the limit ZZ' (the photosphere). We suppose this vapor to be hardly luminous, if at all. The greater part of the beam of white light BO' is only slightly irregularly refracted in it, just as in the other gases to be found there; but those rays whose wave-length differs but slightly from λ_{D_1} or λ_{D_2} are much more deflected, and they may even follow the course indicated by the dotted line BhO. Then from O, at a small distance Ah above A, light may be seen proceeding from B—a source of light with a continuous spectrum—closely resembling sodium light. A spectroscopic examination of this

light, however, will show that it differs more or less from that of the D lines.

It might be thought that only rays with an abnormally high refractive index, *i. e.*, with wave-lengths rather greater than λ_{D_1} or λ_{D_2}, can reach the observer along the path *BhO*. Such, however, is not the case; for if above *A* there were a layer comparable to a prism with the refracting edge perpendicular to the plane of the cut and with base turned upwards, rays with an index smaller than unity must be able to traverse the path *BhO*.

Accordingly in the spectrum of the light that appears outside the Sun's disk we can expect to find rays which are situated on either side of the D lines; perhaps the probability is a little greater for the light on the red side of the absorption lines, because from *A* to *h* the density is more likely to decrease than otherwise.

It is further clear that very near the limb there is the greatest probability of also seeing light that differs relatively much in wave-length from the sodium light; for there a less degree of abnormality of index suffices to deflect rays in the direction of *O*. On the other hand, far above *A*, we can, as a rule, discern only such light as is hardly to be distinguished from D light.

These actually prove to be the principal characteristics of the chromosphere lines. Generally they have a broad base and are arrow-headed. Compare the description and the diagrams to be found in Lockyer's *Chemistry of the Sun*, pp. 109 and 111.

Their typical form appears very strikingly in the hydrogen lines of the chromosphere.

There is no reason to assume that the above considerations, with regard to sodium vapor, do not hold good as well for other gases and vapors. With some of these the anomalous dispersion has been proved already;[1] with others we have been less successful, but the dispersion theories point to its existence in a greater or less degree in all substances.

The characteristic form of the chromosphere lines might, of course, also be accounted for, as is generally done, by the

[1] WINKELMANN, *Wied. Ann.*, 32, 439.

strongly radiating luminous gases and metallic vapors which are thought to be present in the chromosphere, and of which the density near the photosphere must then be taken to be very considerable and to be rapidly decreasing at greater distances. The observed light would then be emitted by those glowing vapors.

Our view of the origin of the chromosphere light does not by any means preclude the possibility of this light owing its existence, partly at least, to self-radiation of incandescent gas; what we have shown is that it may also be refracted photosphere light. Further investigation of the various phenomena of the Sun must decide which explanation goes farthest in considering the whole subject.

Sometimes the chromosphere lines appear under very singular forms, with broadenings, ramifications, plumes, detached parts, etc.[1] Thus far this has been accounted for only on the principle of Doppler, viz., by assuming that the radiating gases move towards, or away from us with tremendous velocity—even as much as 200 km per second and more. Astronomers are all agreed that this explanation is open to many objections, of which we need not remind the reader here.

Beside Doppler's principle, however, we find in the anomalous dispersion another, according to which a gas has the power to originate, under certain circumstances, light differing in wave-length from the characteristic rays of that substance.

Let us suppose, for example, that at some distance above the Sun's limb there is a quantity of hydrogen, with great varieties of density in some of its parts. It will emit not only its own characteristic light, but will, here and there, also deflect earthwards the photosphere light of adjacent wave-lengths. This will, of course, manifest itself in excrescences or ramifications of the hydrogen lines, or as isolated light patches in their neighborhood.

This phenomenon may be expected especially when the slit is adjusted for the examination of prominences where violent

[1] *Cf.* LOCKYER, *loc. cit.*, p. 120.

disturbances take place and where, consequently, considerable differences of density occur.

Though the present explanation of these irregularities in the spectrum is based, like the other one, on the hypothesis that violent disturbances in the solar atmosphere go hand in hand with them, yet the tremendous velocities, required when applying Doppler's principle, do by no means follow from it.

A portion, therefore, of all the light that reaches us from chromosphere and prominences *may* be due to self-luminosity of the gases to be found there; but another, and to all likelihood a very considerable portion is refracted photosphere light reaching us in a manner that reminds us of Töpler's well-known "Schlierenmethode." But there is this difference, that in the "Schlierenmethode" every kind of rays emitted by the source helps to bring out the same irregularities of the medium by ordinary refraction; as a rule no color-phenomena are to be seen, the dispersion of most media being small compared with the average deviation of rays. The chromosphere gases, on the other hand, are to be seen in characteristic colors, because they have an exceptionally high or low refractive index for particular sorts of light. In this case the dispersion is great in comparison with the average deviation of the rays.

Momentarily disregarding the self-radiation of the gases in the solar atmosphere we shall — if the slit is radially adjusted — find those chromosphere lines to be longest and brightest which show the greatest anomalous dispersion. We have seen that the two sodium lines show considerable difference in their respective powers to call forth this phenomenon. Let us make the pretty safe supposition that also the different hydrogen lines and the other chromosphere lines show analogous individual differences and we have the explanation why in the chromosphere spectrum some lines of an element are long and others short, and why the relative intensity of the lines of an element is so different in this spectrum from that in the emission spectrum or in the Fraunhofer absorption spectrum. A careful examination of the anomalous dispersion of a great number of substances will, of

course, have to be made before it can be made out in how far our view will account for the facts already known or yet to be revealed in the chromosphere spectrum. Amongst other things it must then appear whether those elements whose lines are most conspicuous in the chromosphere light do actually cause uncommonly great anomalous dispersion—a wide field for experimental research, the exploration of which has only just commenced.

On the other hand, as regards the self-luminosity of gases, Lockyer's ingenious experimental method of long and short lines affords us an invaluable help to investigate what is the influence of the temperature (and the density ?) of the radiating substance on the emission spectrum. So it seems possible to make out by experiment whether it is radiation or refraction to which the different chromosphere lines are most probably due.

This decision ought, of course, to be founded on a most accurate knowledge of the character which each of the spectral lines of the solar atmosphere exhibits in different circumstances. The coming total solar eclipses offer a good opportunity to observe the chromosphere spectrum minutely, little disturbed by the dazzling light of the photosphere. Especially it is to be hoped that some good spectrograms will be obtained with high dispersion apparatus.

Let us now consider from the point of view of anomalous dispersion the well-known "reversing-layer" which in total eclipses causes the so-called "flash." We have seen before that the theory of dispersion assigns anomalous dispersion to all waves whose periods lie near each characteristic vibration-period of a substance; but the amount of the anomalous dispersion may be slight. In such a case the arrows, in an experiment similar to that described for sodium-light, would be short and narrow, but, for all that, of great intensity. If, therefore, such substances exist in the solar atmosphere even at great distances from the photosphere, with irregularities in density similar to those assumed for sodium, hydrogen, etc., the anomalous refraction will betray the presence of those substances merely in the

immediate vicinity of the edge of the Sun's disk, and only during a few seconds at the beginning and the end of the totality of an eclipse.

This view of the subject makes it a matter of course that the lines of the flash should be very bright; for properly speaking it is not chiefly the radiation emitted by the vapors that we observe, but photosphere light of pretty much the same wave-length. Nor is it necessary that the gases in those places should be of extraordinarily great density, or that their presence should be restricted to a thin reversing layer — one of the most mysterious things the solar theory has led up to and one which astronomers have tried to escape in various ways.

The light of the chromosphere and of the flash lines may be symmetrically distributed on either side of the corresponding Fraunhofer lines; if so, they seem to coincide with the latter; but in certain places of the limb the case must arise that the bright lines would appear to have shifted their position with regard to the absorption lines. For in proportion to the distribution of the density of the vapors, it will be, in turn, especially the rays with very great refractive index (on the red side of the absorption lines) and those with very small refractive index (on the violet side of them) that are curved towards us.

As, upon the whole, the density of the gases of the solar atmosphere will decrease rather than otherwise in proportion as they are farther from the center, it may be expected (according to what we observed with regard to Fig. 4) that the bright lines will oftener shift their position with respect to the Fraunhofer lines in the direction of greater wave-lengths than in that of smaller.

These details will probably become clearly visible in the eclipse-photograms obtained with slit-spectrographs with great dispersion. It is not impossible that in many of the chromosphere lines a dark core may be seen.

Summarizing what we have said, we maintain the following position with respect to that part of the solar atmosphere situated outside what is called the photosphere.

The various elements whose presence in that atmosphere has been inferred from spectral observations are much more largely diffused in it than has generally been assumed from the shape of the light phenomena; they may be present everywhere, up to great distances outside the photosphere, and yet be visible in a few places only; their self-radiation contributes relatively little to their visibility (with the possible exceptions of helium and coronium); the distances, at which the characteristic light of those substances is thought to be seen beyond the Sun's limb are mainly determined by their local differences of density and their power to call forth anomalous dispersion.

In conclusion I wish to say a few words concerning phenomena presented by the Sun-spots. In the spectrum of these spots many of the Fraunhofer lines appear considerably broadened (see, *e. g.*, the diagram in Lockyer, *Chemistry of the Sun*, p. 100). The cause for this has been sought in the presence of very dense absorbing gases, and the broad bands have been attributed exclusively to absorption. The question is whether the second conclusion that we have drawn from the phenomena of refraction in a sodium-flame (p. 190) is not applicable here.

We proceed from the opinion that in a Sun-spot are found great differences of density dependent on strong vertical currents or, according to Faye, on vortex movements in the atmosphere. The phenomenon is commonly localized in the level of the photosphere, at all events, not far above or below it. Now if the entire body situated within the photosphere actually forms a sharp contrast with the outer atmosphere, and if its surface radiates to every side an almost equally intense light with a continuous spectrum, the broadening of the Fraunhofer lines and the darkness of the spots cannot be accounted for by merely attributing the spots to differences of density. The phenomenon must then be set down to differences of temperature, smaller radiating power, condensation, stronger absorption, etc.

Matters are different, however, if A. Schmidt's view is taken to be the correct one, according to which the Sun's limb is an

optical illusion caused by regular refraction in a gradually dispersing, nowhere sharply bounded, mass of gas. In this theory the apparent surface of the photosphere is merely a critical sphere, characterized by its radius being equal to the radius of curvature of rays of light traveling along its surface horizontally; there is not the least question of any discontinuity in the distribution of matter on either side of this spherical surface; inside as well as outside the critical sphere the average density of matter and its radiating power increase gradually towards the center, and it is only at great depths that the condition of matter need be such as to emit a continuous spectrum.

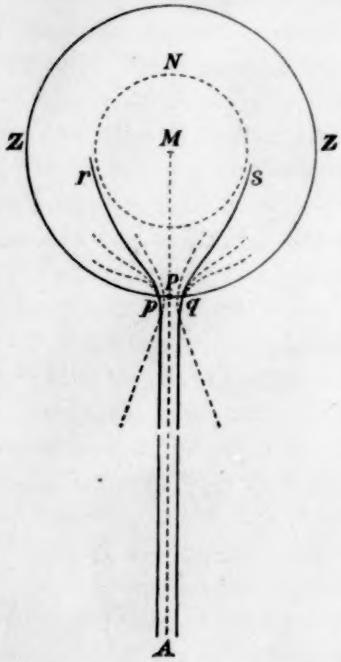


FIG. 5.

Let the circle ZZ' in the diagram (Fig. 5) be the section of the critical sphere with the ecliptic, and let the Earth be in the direction MA. Suppose a spot visible in the center of the Sun's disk; it is seen projected on the critical sphere in P. Now let us suppose that the density increases all around from the center P of the spot, locally producing there cylindrical, rather coaxial, layers with the line of vision for basis. Rays pA and qA suffering normal refraction, may, as is easily seen, have traversed in the Sun the paths rp, sq, and may, therefore, originate, not, it is true, from the most luminous center, but yet from pretty intensely radiating parts of the Sun. They yield the white light of the umbra and of the penumbra, which, though standing out dark against the other parts of the Sun, yet are relatively bright enough.

Slight irregularities in the distribution of density around P render it possible that parallel to PA there emerge rays that have

followed other paths which, nevertheless, will essentially be included in the solid angle *rPs*.

But rays which have undergone anomalous dispersion and yet reach our eye in a direction parallel to *PA*, must have proceeded from a much greater diversity of directions, and need not, therefore, have been emitted in such numbers by the intensely luminous central part of the Sun.

We may also put the matter thus: Of all the light, coming from the intensely radiating nucleus of the Sun (to which may be reckoned all that lies within the sphere *N*) and emerging from the vicinity of *P*, those rays, whose refractive index is abnormally high or low will be more effectually dispersed in all directions, owing to the local differences of density, than rays with a normal index.

The consequence is that the observer, looking in a given direction towards *P*, will see less of those abnormally refracted rays than of the other light. Those rays will, therefore, seem absent in the spectrum of the spot: the Fraunhofer line is seen broadened.

Whereas our considerations concerning the chromosphere light were made independently of any theory of the nature of the photosphere, the present broadly outlined explanation of the phenomenon of the Sun-spots is to a certain extent based on the theory of Schmidt — with which, in fact, it stands or falls.

If subsequent investigations should prove the lines that generally appear broadened in the spectra of the spots, and those which call forth strong anomalous dispersion, to be identical, this would support Schmidt's solar theory.

For the rest it is easy to see that henceforth the principle of anomalous refraction will have to be considered side by side with that of Doppler in every instance when an explanation is required of the many irregularities that have been observed in certain Fraunhofer lines, both near the Sun's limb and in faculae and spots; *cf.* the illustrations in Lockyer's *Chemistry of the Sun* pp. 122 and 123; Young, *The Sun*, pp. 157 and 210; Scheiner, *Die Spectralanalyse der Gestirne*, p. 349.

Such phenomena *may* be caused by refraction, whereas hitherto the only possible explanation was sought in the assumption of tremendous velocities in the line of vision.

The foregoing considerations may suffice to show that anomalous dispersion naturally accounts for a great number of solar phenomena. At any rate no future theory of the Sun can ignore the laws of refraction.

NOTE ON INQUIRIES AS TO THE ESCAPE OF GASES FROM ATMOSPHERES.[1]

By G. Johnstone Stoney.

We have now three investigations which profess to supply information about the escape of gases from atmospheres. Two of them, those of Messrs. Cook and Bryan, reason forward by the help of the kinetic theory of gas from the supposed causes; the third, which is that preferred by the present writer, reasons backward by the help of the same theory from the observed effects.

Mr. Cook's investigation, which will be found in the Astrophysical Journal for January 1900, seeks to compute the proportion of molecules which can attain the speed requisite for escape by means of the formula which Maxwell published in 1860, assigning the proportion of particles whose speed lies between v and $v + dv$, in a system of colliding particles intended to represent an isotropic portion of gas.

Professor Bryan's investigation[2] is based on the investigations made since 1866 into the way in which energy tends ultimately to be partitioned among the various motions possible within a self-contained dynamical system of bodies. The system need not be isotropic, since the bodies may be moving in a constant field of force.

An inquiry by the present writer into Mr. Cook's method of dealing with the problem is attempted in the May and June numbers of the Astrophysical Journal for 1900, and in the present paper a similar attempt is made with reference to Professor Bryan's.

Both Mr. Cook and Professor Bryan predict the proportion of molecules which can escape from an atmosphere by deducing the proportion from its supposed causes, and in this respect are in contrast with an investigation previously published, which

[1] Read before the Royal Society on June 21, 1900.

[2] *Proc. Roy. Soc.*, April 5, 1900, p. 335.

sought to ascertain from the observed effects of escape where and on what scale it has in fact taken place.[1]

Where, as in the present instance, the *a priori* and *a posteriori* methods have led to inconsistent numerical results, there must be a mistake or mistakes somewhere, and it is incumbent upon us to search till these are detected. If they can be found and corrected an important advantage will be gained. Professor Bryan, at the end of his letter in *Nature* of June 7, 1900, indicated one place where a mistake may have been made, viz., in the assumed relation between temperature and the kinetic energy of the translational motions. Another mistake may perhaps have been made in assuming the legitimacy of treating the partition of energy when molecules move in a field of force, as though the only partition to be considered is between these molecules, whereas no field of force can exist unless it has been produced by some physical agent, upon which every motion that goes on within the field must react. In consequence of these reactions no field of force in which any motion occurs can be accurately constant, and a partition of energy based upon the supposition of its constancy is a theorem in rational dynamics, but has no counterpart in nature.

Thus, in the case of the Earth's atmosphere, the anisotropic condition of its outer layers is due to the field of force which exists in the neighborhood of the Earth; and when we are obliged to take into account this anisotropic condition, *as we must when dealing with the escape of gases from atmospheres*, this is to be done (when we are treating the problem as one of partition of energy) by including as molecules between which the partition has to take effect not only the gaseous molecules, but also all the other attracting molecules which provide the field of force.

[So again with reference to the never-ceasing turmoil which goes on in the atmosphere, which near the surface of the Earth exhibits itself in tempests, thunderstorms, and other phases of weather, and which in the upper regions includes phenomena

[1] See memoir by the present writer in the *Scientific Transactions of the Royal Dublin Society*, **6**, Part 13, or in this JOURNAL, January 1898. And for further evidence that helium is escaping from the Earth see *Nature* of May 24, 1900, p. 78.

still more extensive and swift. It is manifest that these events increase the opportunities which gaseous molecules have of escaping from the Earth, and that accordingly *they must be taken into account*, either explicitly or implicitly, in every valid inquiry as to the rate of escape.

To take them into account in an investigation based on the partition of energy, we have to extend that partition to whatever agency produces the turmoil. Now the activity within the atmosphere (and in fact almost every molar activity upon the Earth other than the little which is attributable to tidal action or to such minor agencies as earthquakes and volcanoes) is caused by the shiftings about of energy which comes in between the continuous advent of energy by radiation from the Sun, and its continuous escape from the Earth by radiation into space. Hence, to render an investigation by the Boltzmann-Maxwell law valid, it is necessary to extend the partition of energy beyond the atmosphere — first to the solid Earth, so as thereby to take account of the anisotropic character of some of the atmospheric strata (which facilitates the escape of gas); and secondly to embrace at least the Sun and the ether between the Earth and Sun, so as thereby to take into account the turmoil in the upper regions of the atmosphere (which further increases the rate of escape). It seems to be only when these extensions shall have been effected that a generalized law such as the Boltzmann-Maxwell law for the partition of energy between the various degrees of freedom can become competent to furnish any information with reference to the rate at which gaseous molecules actually do escape from the Earth.—July 17, 1900.]

Then as regards temperature. The temperature of a solid is in reality twofold: it is either its radiation temperature or its conduction temperature. These are physically distinct, although in all but some exceptional cases they are so nearly proportional to one another that they may be given the same mathematical expression. So, again, when dealing with gases we do well to keep in mind the essential distinction between radiation temperature and what may be called convection temperature. The

temperature of an isolated gaseous molecule moving by itself through space is of the first kind only, and depends exclusively on the energy of the internal motions—those motions within the molecule which enable it to absorb or emit radiant heat—and *it is in no degree affected by the kinetic energy* of the translational motion of the molecule; whereas if the same molecule form part of a gas, it meets with encounters with other molecules or with the walls of a containing vessel, and at each such encounter there is a partition of energy between the translational and the internal motions, and in consequence of this the kinetic energy of the translational motion becomes a part of what determines that average power of absorbing and emitting radiant heat which (when estimated over a time embracing a sufficient number of encounters) is the proper definition of the radiation temperature of the molecule. Accordingly the average kinetic energy of the translational motions of the molecule enters into its mathematical expression. If the gas be dense, encounters are frequent, and Δt, the time requisite for the averages, may be brief. In this case the radiation temperature of a molecule, while the gas is undergoing some change in its condition, is predominantly the outcome of its encounters, and depends mainly on the molecules that surround it; whereas if the gas be very much attenuated, then the radiation temperature of the molecule during a period of transition will depend mainly on what influences then reach it from the surrounding ether, and will be but in a subordinate degree affected by the encounters to which the molecule at about that time happens to be subjected.

This is a matter which needs to be very fully taken into account when we attempt to estimate the escape of molecules from the Earth's atmosphere, inasmuch as a large part of the heat radiated by the Sun to the Earth is absorbed by the gaseous molecules which happen at the time to be moving about in those strata of the atmosphere from which alone there can be any effective escape. Accordingly it will need to be carefully scrutinized whether this has been either explicitly or implicitly taken into account in the attempts which have been made to determine *a priori* the rate of escape.

When the molecules of a gas or of a mixture of gases move in a field of force such as that surrounding the Earth, convection currents can exist, and the term temperature as applied to the gas becomes ambiguous. It may have either of two distinct meanings, one of which has reference to the transport of heat by convection and by the consequent sweeping of successive portions of gas against bodies immersed in it, and the other has reference to the exchanges of heat by radiation with those or with more distant bodies. These are different physical events, and the assumption that they stand in a fixed ratio to one another is convenient, but is often not true. It is probably legitimate to regard it as approximately holding good in a gas which has nearly reached a final, *i. e.*, an unchanging condition, and where the problem with which we are dealing does not need our making any closer scrutiny than as to what, on the average, happens to a sufficiently large swarm of molecules within a sufficiently long duration; but it is not true while gas is passing through transition stages, nor is it true of individual molecular events, or of small swarms of events, even in gas which has reached its final state.

Now, none of the gases of the atmosphere have even approached any such state. Changes incessantly go on in the open air at the bottom of the atmosphere, and the extent and abruptness of the changes that as incessantly go on in its upper regions are probably greater.

Again, the consequences of cumulative effects arising in the illimitable trains and combinations of encounters that are taking place, and of associated events in the ether, will also need to be either explicitly or implicitly taken into account in any valid investigation of the escape of gases from atmospheres by the deductive method.

All the circumstances that have been referred to would have to appear among the data of an ordinary dynamical investigation of the escape of an individual molecule from an atmosphere, if such an investigation were possible; and the claim of a generalized theorem like that of the partition of energy to

render it unnecessary to go into these details, ought to be carefully scrutinized. In one case at least the claim does not appear to stand this test, viz., in reference to the supposed legitimacy of the assumption that the field of force surrounding the Earth is constant. Though its variations are minute they are none the less real and are due to interactions between each gaseous molecule and all the molecules of the solid Earth, as real as are the interactions between gaseous molecules when they encounter, and as much entitled to be taken into account, when we seek to carry on the investigation in the region of generalized propositions. It should be kept in mind that in reference to what happens within this region, the plea of being so minute as to be of negligible amount is not admissible. Whether a very small factor may or may not be neglected must be determined independently in each individual case ; and in the above instance the decision is that it may not be neglected.

Other corrections might be suggested along with the principal ones noticed above — that relating to the two kinds of temperature, that relating to the field of force, and that relating to turmoil in the atmosphere; but what seems most to be wanted is that we should recognize that any law for a distribution of energy within the atmosphere by itself, can only come approximately into practical effect after the lapse of a sufficient duration, and throughout a column of the atmosphere from which accidents are excluded ; and that this law will not be the Boltzmann-Maxwell law, which may not be so restricted.

Thus, let us imagine a cylinder like a great Tower of Babel, reaching to the top of the atmosphere, with walls competent to intercept dynamical, electrical, and all other extraneous influences other than gravitation. The air within this tube would consist of molecules, moving in a field of force caused mainly by the Earth's attraction and rotation, and this column of air might perhaps after some such period as a month, a year, a century, or a thousand years nearly attain such a distribution of energy as that indicated by some law. But if, while this process is maturing, a wind overthrows the tower, sweeping away the air it contained

and substituting other air under new conditions, and subject to all the chances of uprushes, downrushes, thunderstorms, auroras, cyclones, cloud, sunshine, rain, etc.; then after all or any of these or of the like accidents, the tower would have to be rebuilt before any portion of the atmosphere extending from the bottom to the top could find itself in a position even to commence the first steps of an advance towards at some future time complying with the law.

The supposition then that the Boltzmann-Maxwell law can be restricted within our existing atmosphere would appear to be a mistake; and if so the inferences from that law are not part of a real interpretation of nature. It need not, therefore, be matter of surprise that, in the case of helium, the facts of nature seem to negative those inferences.

The weather which will prevail over the Earth this day month will be the outcome of the present molecular state of the Earth, and of the molecular events which will happen in the meantime; but our power of stating in mathematical form the existing state of the Earth, and our knowledge of molecular physics, are not such as would enable us to predict that future weather by the *a priori* or deductive method of proof. The difficulties in this case are easily seen; and they are instructive, since the escape of gas from the Earth depends on phenomena which are probably as complex as those which determine the weather and as little amenable to treatment by the deductive method.

Any such distribution of energy as that assigned by the Boltzmann-Maxwell law would, if it could be realized, be brought into existence by the gradual effacement of excesses which had previously existed; from which it would appear to follow that excesses prevail in our existing atmosphere greater and more numerous than could exist in an ideal atmosphere that obeyed that law. It is probable, therefore, that in our actual atmosphere there are more opportunities for the escape of molecules than there would be in the ideal atmosphere — a conclusion which accords well with the fact that the actual rate of escape exceeds those computed by Professor Bryan and Mr. Cook.[1]

[1] See *Nature* of May 24, 1900, p. 78, second column.

THE COMPLETE EMISSION FUNCTION.

By P. G. Nutting.

The Wien[1]-Paschen[2] formula representing the emission of perfectly radiating bodies in its relation to their temperature and the period of the emitted radiation has been quite thoroughly tested in the region of the continuous spectra. Wanner[3] has shown the possibility of its holding, in the case of an enclosed source, even in the sodium line-spectrum. But to represent the free emission of imperfect radiators, in whose emission spectra there are maxima independent of the temperature at particular wave periods, the formula is quite inapplicable. If, for instance, we attempt to calculate the temperature of the Sun, using the above mentioned formula and the amount of radiation of a given wave period received at the Earth, we arrive at the absurd result, as Planck has shown,[4] of a different solar temperature corresponding to the emission of each wave period used in the calculation. It has seemed worth while to construct an emission function of wave period and temperature which should hold for the free emission of all substances at all temperatures, as well in the line as in the continuous spectrum. The theorems and methods of the modern theory of functions will be used in its construction.

So many terms are in use relative to emission and emitting power, that we may avoid confusion in what follows by specifically stating the meaning of the terms employed. In all cases the temperature of the emitting body is supposed maintained, no law of cooling being concerned. Unlike *surface conductivity*,[5] these terms take no account of convection, conduction, or of

[1] W. Wien, *Wied. Ann.*, **58**, 662, 1896.

[2] F. Paschen, *Wied. Ann.*, **58**, 491, 1896.

[3] H. Wanner, *Wied. Ann.*, **68**, 143, 1899.

[4] M. Planck, *Ann. d. Ph.*, **1**, 722, 1900.

[5] T. Preston, *Theory of Heat*, London, 1894, p. 443.

radiation received from surrounding objects; thus assuming emission in a vacuum with non-reflecting surrounding objects at the absolute zero of temperature. If the emission be found to be a varying function of the pressure, or of the density of the emitting body, then these are supposed constant, or else the emission corrected for their variations. Each of the five quantities defined applies as well to the total integral emission of waves of all periods as to the particular emission of waves of one period. The former is the integral of the latter from zero to infinity with respect to the period in all cases. The defining equation of each quantity defined may be easily written.

The *emission* from a body is the energy leaving its whole surface by radiation in a unit of time. It is the integral of the specific emission over the surface. We may consider either the *total* emission of all wave periods or the *particular* emission of a single wave period.

The *specific emission* of a body is its emission per unit area of surface. It is the energy leaving each square centimeter of its surface by radiation, in each second, in all directions. This appears to have been the unit used by Paschen.

The *intensity of emission* is the specific emission from a body perpendicular to its surface, the energy radiated normally from each square centimeter of the surface of a body in each second. Consider an imaginary tube whose walls are normal to the radiating surface, and which cut unit area from it. Then the intensity of emission is measured by the energy traversing any cross-section of the tube in a second, only that radiation being considered which never crosses the walls of the tube. Cotton, in his discussion of Kirchhoff's law,[1] apparently assuming Lambert's law, defines intensity of emission as measured by the total energy contained at any one time within an imaginary cylindrical tube of unit section, cutting the emitting surface at any angle.

The *emission constant* for any given temperature and for any particular wave period or group of wave periods, is the intensity

[1] A. COTTON, *Rev. Gen. des Sciences*, February 15, 1895. Also this JOURNAL, 9, 250, 1899.

of emission from a black or perfect radiator. Where observed data are lacking or conflicting, the validity of the Wien-Paschen formula is assumed, together with the values of the constants determined by Paschen.

The *emitting power* of a substance is the ratio of its intensity of emission to that from a perfect radiator at the same temperature and under the same conditions. Like reflecting and absorbing power as commonly defined, its value lies between zero and unity for all substances in all conditions. It is perhaps more easily determined experimentally than any of the other quantities above defined, but intensity of emission is the simpler function mathematically and will be used in this discussion.

The intensity of emission being a function of the entirely independent arguments temperature and wave period, we may construct each function separately and then combine them in any manner such that each argument shall enter the function of the other as a parameter without affecting its form.

Consider first the intensity of emission as a function of the temperature. So far as we know from experimental evidence, the function is holomorphic over the whole region, for all substances in all conditions, and for all wave periods. It is continuous and single-valued in both argument and function throughout. Having no roots other than zero, it cannot be an integral, algebraic polynomial. Having no poles, it cannot be a fractional, algebraic function. It is obviously not polygenic, elliptic, nor automorphic. It cannot be a circular function, for it is not periodic, nor has it finite zeros. The remaining exponential function does satisfy the conditions. We reject the direct exponential ae^{bT} as giving a finite value at zero. We have, then, the inverse exponential function

$$E = ae^{-b/T}, \qquad (1)$$

satisfying all the above conditions. We reject the more general form, in which the second member is a sum of similar terms, as being unnecessarily complicated for expressing all the conditions at present known.

Either a or b, or both, may contain functions of the wave period. In the complete function, they may be found to contain, also, some function of the temperature which makes the emission decrease very rapidly above the temperature of vaporization, but we have yet no experimental evidence that it is necessary to impose such a condition. The condition cannot be imposed without making the function very much more complicated in form. If the intensity of emission, corrected for differences in volume or pressure, be found to decrease rapidly at temperatures above the temperature of vaporization only for waves of a certain period, the fact will be taken account of by a period parameter, and not a temperature parameter, in the coefficient a.

The intensity of emission as a function of the wave period is continuous and is single-valued in the argument for all substances, with all surface conditions, at all temperatures. It has the value zero at zero and infinity, and only at these points. For a perfect radiator it has a single maximum which varies with the temperature. As with the emission-temperature function, we are again limited to the inverse exponential, but with the factor τ^{-n}, where τ is the period of vibration, this will make the function vanish for very long waves.

For a perfect radiator we have then

$$E = a\tau^{-n} e^{-\frac{\beta}{\tau}}, \tag{2}$$

satisfying the general conditions imposed. Combining the second function with the first, we have the familiar

$$E = A\tau^{-n} e^{-B/\tau T}, \tag{3}$$

holding for all substances having but a single maximum, and that varying with the temperature. Its maximum value

$$E_m = A\,(n/Be)^n\, T^n \tag{4}$$

corresponds to a period $\tau_{max} = B/nT$. The total intensity of emission at any temperature, which is the integral of (3) with respect to the wave period, is easily obtained[1] by integrating it as an

[1] This integration is given by PASCHEN, *Wied. Ann.*, **60**, 666, 1897.

Eulerian integral of the second kind. The total intensity of emission is thus

$$E_0 = AB^{1-n}\,\Gamma(n-1)\,T^{n-1}\,, \tag{5}$$

where $\Gamma\,(n-1)$ denotes the ordinary gamma function.

For the particular value $n = 5$, formula (3) is similar to Wien's, and (5) expresses Stefan's law.

Evidently equation (3) cannot represent the free emission from an imperfect radiator, in whose isothermal emission curves there are maxima not varying in position with the temperature.

The emission from rock salt, for instance, is very largely confined[1] to the region in the immediate vicinity of 50μ. In order to represent the emission from such a substance, we must interpose in (3) a sharp maximum or a pole. This is most simply done by writing

$$E = A\,(\tau - \tau_m)^{-n}\,e^{-B/\tau T}\,. \tag{6}$$

This satisfies all the conditions imposed on (3) except that it is meromorphic, having the constant polar maximum, τ_m. We cannot put the polarity in the exponential in any simple form without violating the conditions imposed. But E must be always real and positive, and this condition limits n in (6) to even whole numbers. That n may be restricted only to real positive values, we may write instead of (6),

$$E = A\tau^{-n}\,(\tau - \tau_m)^{-2}\,e^{-B/\tau T}\,. \tag{7}$$

Formula (7) should hold for any substance having but a single maximum of constant period. But most substances have more than one such emission maximum (spectrum line). For substances having m such maxima, (7) is generalized by writing

$$E = A\tau^{-n}\,e^{-B/\tau T}\,\Sigma_{m=1}^{m}\,(\tau - \tau_m)^{-2}\,, \tag{8}$$

holding in general for the free emission of all substances. In its still more general form, each polar fraction, $(\tau - \tau_m)^{-2}$, might have a different coefficient, but we have not sufficient data at present to determine whether or not this generalization is necessary. We may note in passing that these polar maxima $\tau_1 - - - \tau_m$, of the emission-period function, are identical with

[1] E. Aschkinass, *Ann. d. Physik*, 1, 60, 1900.

the poles of the reflection-period and absorption-period functions, and that the poles of the refraction-period function are included among them.

If the polar maxima τ_m were absolute constants, (8) would give the emission an infinite and therefore too large value for $\tau = \tau_m$. But the finite width of the spectrum lines shows that we must regard τ_m (which may perhaps be identified with molecular or atomic period) as varying rapidly and continuously within certain very narrow limits. Suppose we replace τ_m by $\tau_m + a \cos k$, where a is an absolute constant and the time derivative of k is very large. Then the polar maximum has all values from $\tau_m + a$ to $\tau_m - a$ in rapid succession, so that if the observation of the emission extend over any considerable interval of time, it will not appear large. Even a very small value of a brings the emission maximum down to a moderate value. When a is large, the emission maximum is low and broad. In a very general way a appears to be proportional to τ_m, that is, for a given substance having a number of spectrum lines, those lines of greatest period are often the widest. And the gases of small molecular weight often have narrower and more sharply defined maxima even in the infra-red than substances of greater molecular weight.

Formula (8) agrees very well with the scant data at present available. It gives a curve closely resembling those obtained by Rosenthal[1] and by Rubens and Aschkinass.[2] For periods much greater than τ_m and in general for low temperatures, the effect of the presence of the polar constant is vanishingly small and (8) reduces to (3). Nearer τ_m and within a few octaves of it, the emission given by formula (8) is much less than that given by (3), using the same constants. The defect in the Wien-Paschen formula in this region was noted by Rosenthal. On the side of the shorter waves, the polar period τ_m reduces the emission much more rapidly to a much smaller value than would be given by the formula lacking it. At a distance of a few octaves from

[1] ROSENTHAL, *Wied. Ann.*, **68**, 783, 1899.

[2] RUBENS and ASCHKINASS, *Wied. Ann.*, **64**, 589, 1898.

any τ_m on the side of the shorter waves, the whole coefficient of the exponential becomes practically a constant and the emission is very slight.

In other words, by formula (8) the emission of any body at a distance of a few octaves from its constant maximum of greatest period, on the side of the greater periods, is very like that from a black body. It differs from that from a perfect radiator chiefly on account of its surface condition. But on the side of the shorter periods from even the first polar maximum, the emission is entirely different from that from a black body and in that region formula (3) cannot hold for any temperature or any wave periods. Between the spectrum lines and within any considerable distance of any line the emission is very slight, in fact of lower order and practically zero. The total radiation in this region is thus nearly all confined to the lines themselves, at any temperature.

These deductions from formula (8) agree well with known facts. Most metals give for relatively low temperatures and great periods an emission very similar to that from a perfect radiator. Paschen found that even bright platinum radiated sensibly as a black body. But in the region of the line-spectra of the metals, for relatively high temperatures and short periods, the emission is of quite a different character and practically confined to the emission maxima. Formula (8) covers both cases equally well. Even glass and the colorless crystalline salts having emission maxima far in the infra red, emit and absorb very slightly on the shorter period side of these maxima.

BERKELEY, CAL.
May 1900.

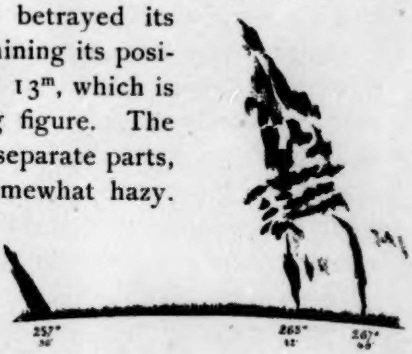
RISE OF A LARGE PROMINENCE ON JUNE 1, 1900.

By J. Fényi, S.J.

After a long period of comparative quiet in the solar atmosphere I now have to report a phenomenon of extraordinary violence which appears to signalize a stormy awakening of renewed activity.

At 2^h Gr. M. T. on June 1, I observed at the Haynald Observatory in Kalocsa a prominence which rose with a rapidity seen only in case of the most violent eruptions, and reached the altitude 431″, which is extremely high for the present epoch of minimum. The special significance of this altitude appears from the fact that the highest prominences of the years 1896, 1897, and 1899 attained only the heights, respectively, of 196″, 197″, and 149″.

In the usual observation of the Sun's limb I found in position-angle 265° 42′ to 267° 40′ a prominence of moderate height whose intense brilliance betrayed its eruptive character. After determining its position, a drawing was made at 2^h 13^m, which is reproduced in the accompanying figure. The prominence consisted of several separate parts, which were very bright and somewhat hazy. The measurement of the height with the filar micrometer was difficult, yielding a very uncertain value of 93″. In order to determine the height with certainty, and to follow the rise of the prominence, I now let it run across the slit, noting also the transit of its lower part, as the two flames which formed the base were already dissolving and disappeared during the transits; so that at the last transit nothing at all was visible up to a height of 188″, or 136,000 km, while the separated parts preserved their forms up to 431″. The results of



the measures are collected in the following table. The first column contains the exact time of the measurement of the vertex; the second gives the height of the prominence calculated from the transits; the third gives the duration of each transit, while the fourth contains the mean velocity, in kilometers per second, of the ascent in the following interval, 1″ being taken as equal to 725 km.

RISE OF PROMINENCE ON JUNE 1, 1900.

Gr. M. T.	Height of vertex	Duration of transit	Velocity	Gr. M. T.	Height of base
2^h 14^m $8^s.3$	289″.85	$21^s.5$	546 km	13^m $57^s.2$	144″.56
14 41 .5	323.87	23 .3	231	14 29 .6	158.46
15 16 .4	334.99	24 .1	371	15 3 .0	148.73
15 51 .7	353.06	25 .4	187	15 37 .3	152.90
16 29 .0	362.72	26 .1	471	16 15 .8	190.43
17 16 .2	393.37	28 .3	423	17 1 .4	187.65
18 3 .8	421.17	30 .3	138	17 47 .2	190.43
18 55 .0	430.90	31 .0		18 37 .5	187.65

The fifth column gives the time when the lower part of the prominence passed across the slit, and the sixth contains the height calculated from this.

I must state in regard to the velocities of ascent in the fourth column that no safe conclusions can be drawn from their differences, as they are a result of the uncertainty of the individual measures from which the figures were calculated. On trying to smooth out the data graphically I found that if corrections of at most $0^s.6$ are applied to the time of transit an entirely uniform ascent with constant velocity is obtained.

Such corrections are entirely justified by numerous transits of prominences, for it is not unusual for a single measure to differ from the mean of many transits by more than $0^s.5$. The assumption of a uniform ascent is unjustified, however, as the prominence is without doubt under the influence of gravity, and it must therefore sink at the time it is rising, unless it is impelled upward by some quite problematic continuous force. I have reduced the observed altitudes on the assumption of constant gravity with an acceleration of 270 meters, and again sought to smooth them

out graphically. I found that the observed times of transit would require a correction of $0^s.7$ in only one case in order to be in full agreement with this assumption. Hence it appears that the above observations, despite the capricious ascent, are by no means inconsistent with the assumption that the prominence is a mass of hydrogen projected from the Sun, which rises above the chromosphere with the velocity of empty space and is under the influence of gravity alone.

But, although we admit of uncertainties of $\pm 9''$ in the single determinations of transits—which would materially modify differences of height of $10''$ to $30''$—we obtain from the whole data the entirely reliable velocity of ascent of 334 km per second; the prominence rose $132''$ in $4^m\ 46^s$; an uncertainty of even $10''$ is of no consequence.

During the ascent of the prominence I also observed a large displacement of the lines of the spectrum, those from the central part of the mass being displaced toward the violet. A rapid setting of the micrometer thread indicated that this velocity was about 350 km per second.

The approaching dissolution was noted during the last transit; the smaller portions were invisible, and the larger somewhat faint and diffuse, unless possibly this was the effect of a temporary haziness of the sky. Three minutes later the entire brilliant prominence had disappeared; no trace of it could be any longer seen even far above the chromosphere. According to the notes made, the duration of the violent eruption would therefore be estimated at only fifteen minutes. This estimate is confirmed when we divide the altitude attained, 312,400 km, by the mean velocity of ascent, whereby we similarly obtain $15^m\ 35^s$.

The rapidity of the dissolution deserves special attention. Under the circumstances an accurate measure of the separate parts was impossible. If I take as a basis the dimensions from the scale of the drawing of the central part and assume that it actually dissolved in the three minutes, as stated above, then a temperature of $10{,}000^\circ$ would not be sufficient—$30{,}000^\circ$ would be needed—to explain its dispersion into empty space in so short

a time. No difficulty is encountered here, nor is it generally met with in case of violent eruptions; but it arises in case of the low-hanging prominences, which dissolve so slowly that we should have to assume a temperature which is far too low.

This unusual phenomenon can be brought into relation with the condition of the solar surface, as a small Sun-spot, surrounded by an extended group of faculæ, was approaching the limb at that point. It is true that the spot was 19° distant on a great circle from the limb; but such eruptions do not commonly proceed from the spot itself, but from its neighborhood, and frequently at about the above distance from it. It is worthy of mention in this connection that after the passing of this remarkable phenomenon I also observed at position-angle 259° a very rapid ascent. At that point I found a very bright flame, inclined 60° toward the pole, therefore diverging from the region of the spots, the rapid rise of which led me to measure its height from minute to minute. It rose from 41″ to 80″ between $2^h\ 31^m$ and $2^h\ 37^m$, when the central portion faded and the vertex separated. But now a new flame of a similar sort rose from the part remaining on the chromosphere, ascended rapidly in the same direction, and finally divided into four parts. The rise between 41^m and $49^m.5$ was from 49″ to 135″; the former prominence, therefore, rose with a velocity of 70 km; the latter with 80 km. Rapid ascents of small flames are not unusual, but the elevation attained was remarkable. The coincidence of these last ascents with the great eruption at the same time in the same region of spots again confirms the view expressed years ago that the phenomena of the spot zone are due to a common cause acting at considerable depths within the Sun.

HAYNALD OBSERVATORY,
June 1900

FIELD OF THE REFLECTING TELESCOPE.

By S. C. Reese.

The writer has made an investigation of the field of the reflecting telescope and has found that there is no field, plane or curved, on which the images of stars off the axis of the paraboloid are free from distortion. Reflectors of short focal length and large aperture accentuate the distortion of star images and have called special attention to the aberration in the focal plane. Tennant[1] some years ago made a thorough investigation of the form of images in that plane; but, so far as the writer knows, Tennant's results have not been generalized, nor has it been shown that there is no field of circular images (where by a circular image we mean, not a diffraction disk, but a single round blurred patch of light). If such a field as the last did exist it would be the size of the round images that would decide the advisability of an attempt to use it in photography, as a small distorted image is better for many purposes than a large circular image. In order to give the subject as general a treatment as seemed required the writer made use of the methods of Kirchhoff[2], changing them to adapt them to the problem in hand. The writer has tried to call attention to the various limitations made and in the cases where the aberration in only a single direction or a single plane is discussed has avoided as far as possible confusing such aberration with that of the general case. No numerical results are given, as the shape and size of the images are well known to those who have photographed with the reflector and used either plane or curved plates.

[1] Tennant, "Notes on Reflecting Telescopes," *M. N.*, 47, 1886–7, pp. 244–256.

[2] Kirchhoff, "Zur Theorie der Lichtstrahlen," *Sitzungsberichte der königlich Preussischen Academie der Wissenschaften zu Berlin* **30**, 641, June 22, 1882. This paper is republished in *Poggendorff's Annalen*, Neue Folge, 1883, **18**, 663, and is used in a slightly different form in Kirchhoff's *Mathematische Optik*.

Since the source of light is at an infinite distance the wave incident on the mirror is plane and the motion at the point x, y, z, can be represented by the equation

$$\phi = A \cos\left(\frac{r_1}{\lambda} - \frac{t}{T}\right) 2\pi , \tag{1}$$

where λ is the wave-length and T the period of vibration of the light, and r_1 the perpendicular distance of the point x, y, z, from a fixed plane $a_1 x + \beta_1 y + \gamma_1 z + p = 0$ parallel to the wave-front, but otherwise arbitrary.

Now, by the use of Kirchhoff's method and the application of Green's theorem[1] we find that the motion ϕ_0 resulting at x_0, y_0, z_0, from a reflection can in most general terms be represented by

$$4\pi \phi_0(t) = \int ds \left[\frac{\delta}{\delta N} \frac{A \cos\left(\frac{r_1}{\lambda} + \frac{t - \frac{r_0}{a}}{T}\right)}{r_0} - \frac{f\left(t - \frac{r_0}{a}\right)}{r_0} \right], \tag{2}$$

where the integral is taken over the entire reflecting surface, and where f is defined by the equation

$$\frac{\delta\phi}{\delta N} = f(t) .$$

In this expression r_0 represents the distance from the point x, y z, to the point x_0, y_0, z_0, and a the velocity of propagation of light. The integral in (2) can be broken up into the two expressions

$$\int - \frac{1}{r_0^2} A \frac{\delta r_0}{\delta N} \cos\left(\frac{r_1 + r_0}{\lambda} - \frac{t}{T}\right) 2\pi ds$$
$$+ \int - \frac{2\pi}{r_0\lambda} \frac{\delta r_0}{\delta N} \sin\left(\frac{r_1 + r_0}{\lambda} - \frac{t}{T}\right) 2\pi ds .$$

Now, λ is an infinitesimal of the first order with respect to r_0; therefore, the first of these integrals becomes infinitesimal with respect to the second and we have to consider only

$$4\pi \phi_0(t) = \int - \frac{2\pi}{r_0\lambda} \frac{\delta r_0}{\delta N} \sin\left(\frac{r_1 + r_0}{\lambda} - \frac{t}{T}\right) 2\pi ds . \tag{3}$$

[1] KIRCHHOFF, pp. 644–646.

It is necessary that when a plane wave falls on a portion of a surface and is brought to a focus x_0, y_0, z_0, by reflection, that that portion of surface should be capable of having close contact with the surface of a paraboloid of revolution having the point x_0, y_0, z_0, for its focus.

Now, the paraboloid of revolution has the property that the distance of each one of its points from the focus equals the distance of that point from a fixed plane (which can be called the "director plane"). Hence, for the paraboloid of revolution the condition is satisfied

$$r_0 + r_1 = \zeta\,, \tag{4}$$

where r_0 and r_1 have the meanings given above and ζ is the perpendicular distance between the arbitrary plane spoken of above and the "director plane."

Relation (4) is necessary to the integration of equation (3) by the method of Kirchhoff. Setting $\frac{2\pi}{\lambda} = k$ and $-\frac{t}{T}2\pi = \delta$, and having regard to (4), we find that (3) becomes

$$4\pi\phi_0 = -\int \frac{k}{r_0}\frac{\delta r_0}{\delta N}\sin(k\zeta + \delta)\,ds\,. \tag{5}$$

If

$$G = -\frac{\frac{\delta r_0}{\delta N}}{r_0}$$

then

$$4\pi\phi_0 = k\int G\sin(k\zeta + \delta)\,ds\,, \tag{6}$$

which can be shown to be equal to

$$4\pi\phi_0 = -\left[\frac{dF}{d\zeta}\cos(k\zeta + \delta)\right]_{\zeta_0}^{\zeta_1} \tag{7}$$

where F is a function whose first derivative is continuous for values of ζ between ζ_0 and ζ_1.

Having obtained this integral, we can commence the discussion of the mirror formed of a paraboloid of revolution. The paraboloid referred to, a rectangular system of axes with the z axis corresponding to the axis of revolution, can be represented by the equation

$$x^2 + y^2 = 4az \tag{8}$$

or, referred to rectangular axes, of which the z axis is normal to the surface, the x axis tangent to a principal section, and the y axis is in the tangent plane perpendicular to both these, the equation of the paraboloid is

$$\cos^2\theta x^2 + y^2 + 2\sin\theta\cos\theta xz + \sin^2\theta z^2 - \frac{4a}{\cos\theta}z = 0\,, \tag{9}$$

where θ is the angle between the new z axis and the axis of the paraboloid. In forming this equation, since the normals of a figure of revolution intersect the axis, no assumption has been made which limits the generality. But if we now consider a small portion of the paraboloid we may take x and y so small that terms of higher than the second order[1] can be neglected, and the equation of the paraboloid becomes

$$\cos^3\theta x^2 + \cos\theta y^2 = 4az\,, \tag{10}$$

or

$$z = a_{11}x^2 + a_{22}y^2\,, \tag{11}$$

where $a_{11} = \dfrac{\cos^3\theta}{4a}$, $a_{22} = \dfrac{\cos\theta}{4a}$, and θ is the angle between the z axis and the axis of the paraboloid. This form (11) is of the greatest importance, as it will appear that for such portions of surface the pencil of reflected light has no faults other than that of pure astigmatism. We may put also

$$ds = dxdy\,. \tag{12}$$

For any determined value of θ the distance of the point x_0, y_0, z_0, toward which the wave converges, from the origin is

$$s_0 = \sqrt{x_0^2 + y_0^2 + z_0^2}\,. \tag{13}$$

The distance of the point x_0, y_0, z_0, from a neighboring point x, y, z, on the surface of the mirror is

$$r_0 = \sqrt{(x - x_0)^2 + (y - y_0)^2 + (z - z_0)^2}$$

which by the use of (11), (13), and the quantities

$$\alpha_0 = \frac{-x_0}{s_0}\,,\quad \beta_0 = -\frac{y_0}{s_0}\,,\quad \gamma_0 = -\frac{z_0}{s_0}\,, \tag{14}$$

[1] KIRCHHOFF, p. 653; Strehl, *Theorie des Fernrohrs* (Leipzig, 1894), p. 11.

becomes to terms of the third order

$$r_0 = s_0 + \alpha_0 x + \beta_0 y + (a_{11}x^2 + a_{22}y^2)\gamma_0 + \frac{1}{2s_0}[x^2(1-\alpha_0^2) - 2\alpha_0\beta_0 xy + y^2(1-\beta_0^2)] .$$

But

$$r_1 = p + \alpha_1 x + \beta_1 y + \gamma_1[a_{11}x^2 + a_{22}y^2] ,$$

where α_1, β_1, γ_1, are direction cosines of the normal to the arbitrary plane, therefore

$$\zeta = (s_0 + p) + \left(a_{11}\gamma_0 + a_{11}\gamma_1 + \frac{1-\alpha_0^2}{2s_0}\right)x^2 - \frac{\alpha_0\beta_0}{s_0}xy + \left(a_{22}\gamma_0 + a_{22}\gamma_1 + \frac{1-\beta_0^2}{2s_0}\right)y^2 , \tag{15}$$

or

$$\zeta = A_0 + A_{11}x^2 + 2A_{12}xy + A_{22}y^2 , \tag{16}$$

where

$$\left.\begin{aligned} A_0 &= s_0 + p \\ A_{11} &= a_{11}\gamma_0 + a_{11}\gamma_1 + \frac{1-\alpha_0^2}{2s_0} \\ A_{22} &= a_{22}\gamma_0 + a_{22}\gamma_1 + \frac{1-\beta_0^2}{2s_0} \\ A_{12} &= -\frac{\alpha_0\beta_0}{2s_0} \end{aligned}\right\} . \tag{17}$$

It can now be shown that the equation (7) takes the form

$$4\pi\phi_0 = -G\frac{\pi}{\sqrt{\mu_1\mu_2}}\cos(kA_0 + \delta) , \tag{18}$$

where μ_1 and μ_2 are the roots of the equation

$$(A_{11} - \mu)(A_{22} - \mu) - A_{12}^2 = 0 . \tag{19}$$

Now

$$\mu_1\mu_2 = A_{11}A_{22} - A_{12}^2 . \tag{20}$$

Whenever this product becomes zero the intensity of light becomes infinite and a focus is reached. The equation for determining the focus is, then,

$$\left[a_{11}\gamma_0 + a_{11}\gamma_1 + \frac{1-\alpha_0^2}{2s_0}\right]\left[a_{22}\gamma_0 + a_{22}\gamma_1 + \frac{1-\beta_0^2}{2s_0}\right] - \frac{\alpha_0^2\beta_0^2}{2s_0^2} = 0 , \tag{21}$$

a quadratic equation in s_0.

Since γ_1 is the cosine of the angle the incident ray makes with the normal, and θ is the angle that the normal makes with the axis, then if C denotes the angle the incident ray makes with the axis of the paraboloid, we have the relation

$$\cos^{-1}\gamma_1 + \theta = C,$$

or

$$\gamma_1 = \cos(C - \theta)\,. \tag{22}$$

To determine whether the foci for the same ray reflected at different parts of the mirror fall at the same place or different places it is necessary to consider only some particular cases. The aberration shown by these determines qualitatively what may be expected in the general case.

I. When the incident light is parallel to the axis of the paraboloid. In equation (10) $\theta = 0$ and in (17) $\gamma_1 = 1$. Hence (21) becomes

$$\left[\frac{1}{4a}\gamma_0 + \frac{1}{4a} + \frac{1-\alpha_0^2}{2s_0}\right]\left[\frac{1}{4a}\gamma_0 + \frac{1}{4a} + \frac{1-\beta_0^2}{2s_0}\right] - \frac{\alpha_0^2\beta_0^2}{4s_0^2} = 0\,. \tag{23}$$

For the vertex of the paraboloid

$$\alpha_0 = \beta_0 = 0 \quad \gamma = 1 \quad \therefore \quad s_0 = -a\ ;$$

that is, the focus of the light parallel to the axis incident at the vertex is at the geometrical focus of the paraboloid. The sign shows the reversed direction of the light. If in (21) we set $\beta_0 = 0$ (that is, consider only a principal section) and in (22) set $C = 0$, then since $\gamma_0 = \gamma_1$ and $\alpha_0^2 + \beta_0^2 + \gamma_0^2 = 1$ we have

$$\left[\frac{\cos^4\theta}{4a} + \frac{\cos^4\theta}{4a} + \frac{\cos^2\theta}{2s_0}\right]\left[\frac{\cos^2\theta}{4a} + \frac{\cos^2\theta}{4a} + \frac{1}{2s_0}\right] = 0\,,$$

or

$$s_0 = -\frac{1}{\cos^2\theta}\cdot a = -\frac{a}{\cos^2\frac{1}{2}\omega}\,,$$

where ω is the angle between the reflected light and the axis of the paraboloid. This value of s_0 is the same as the distance of a point on a parabola from the geometrical focus,[1] so that we have here the proof that the light parallel to the axis of a parabola,

[1] See equation 6, Poor's "Aberration of Parabolic Mirrors," this JOURNAL, 7, 114, 1898.

and reflected from any point of the figure, comes to a focus at the geometrical focus of the figure.

II. When the incident light makes an angle C with the axis of the paraboloid. (1) When only the central part of the mirror is considered.

$$\gamma_0 = \gamma_1 = \cos C.$$

If we again consider a principal section we have $\beta_0 = 0$, and (21) becomes

$$\left[\frac{\cos C}{2a} + \frac{\cos^2 C}{2s_0}\right]\left[\frac{\cos C}{2a} + \frac{1}{2s_0}\right] = 0.$$

Then

$$s_0 = -a \cos C, \qquad (24)$$

that is, the focus lies on the circle of diameter a through the vertex and focus (in the case of the solid this is a sphere), or

$$s_0 = -\frac{a}{\cos C}. \qquad (25)$$

The second focus of the pencil lies on the line through the geometrical focus perpendicular to the axis (in case of the solid this is the focal plane).

The fact that the two foci do not fall together is an immediate consequence of the astigmatism of the pencil.

Between these two foci is the place of the circular image, which is not a true focus. For this to the first approximation[1]

$$s_0 = -\frac{a(1+\cos^2 C)}{2\cos C}. \qquad (26)$$

The z's of the points given by (24), (25), and (26) are given by

$$\left.\begin{aligned} z &= -a\cos^2 C; & (27)\\ z &= -a; & (28)\\ z &= -\frac{a(1+\cos^2 C)}{2}; & (29)\end{aligned}\right\} A$$

while the x's are given by

$$\left.\begin{aligned} x &= -a\cos C\sin C; & (30)\\ x &= -a\tan C; & (31)\\ x &= -\frac{a(1+\cos^2 C)}{2}.\tan C. & (32)\end{aligned}\right\} B$$

[1] STREHL, *Theorie des Fernrohrs*, Article 11, p. 17.

(2) When the portion of the paraboloid on which the light falls normally is considered.

In equation (21)

$$\gamma_0 = \gamma_1 = 1\,, \quad a_{11} = \frac{\cos^3\theta}{4a}\,, \quad a_{22} = \frac{\cos\theta}{4a}\,.$$

If we again consider a principal section (which the coördinates we use allow us to take through the point of incidence), again $\beta_0 = 0$ and (21) becomes

$$\left[\frac{\cos^3\theta}{4a} + \frac{\cos^3\theta}{4a} + \frac{1-\alpha^2}{2\mathfrak{s}_0}\right]\left[\frac{\cos\theta}{4a} + \frac{\cos\theta}{4a} + \frac{1}{2\mathfrak{s}_0}\right] = 0\,. \tag{33}$$

But since

$$\alpha_0^2 + \beta_0^2 + \gamma_0^2 = 1\,, \quad \alpha_0 = 0\,.$$

Hence

$$\mathfrak{s}_0 = -\frac{a}{\cos^3\theta} \qquad (34) \qquad \text{or} \qquad \mathfrak{s}_0 = -\frac{a}{\cos\theta}\,. \qquad (35)$$

For circular images

$$\mathfrak{s}_0 = -\frac{a\,(1+\cos^2\theta)}{2\cos^3\theta}\,. \tag{36}$$

To find the z and x for each of these points we must use the formulae

$$\left.\begin{aligned} -z &= a\tan^2\theta + [\mathfrak{s}_0]\cos\theta\,, \\ -x &= -2a\tan\theta + [\mathfrak{s}_0]\sin\theta\,, \end{aligned}\right. \tag{37}$$

where $[\mathfrak{s}_0]$ is the value of $\mathfrak{s}_0$ apart from sign.

The equations (34) (35), and (36) give

$$\left.\begin{aligned} z &= -a\tan^2\theta - \frac{a}{\cos^2\theta} && (38) \\ z &= -a\tan^2\theta - a\,, && (39) \\ z &= -a\tan^2\theta - \frac{a\,(1+\cos^2\theta)}{2\cos^2\theta}\,. && (40) \end{aligned}\right\} C$$

$$\left.\begin{aligned} x &= 2a\tan\theta - \frac{a}{\cos^3\theta}\sin\theta\,, && (41) \\ x &= 2a\tan\theta - \frac{a}{\cos\theta}\sin\theta\,, && (42) \\ x &= 2a\tan\theta - \frac{a\,(1+\cos^2\theta)}{2\cos^3\theta}\sin\theta\,. && (43) \end{aligned}\right\} D$$

From equations A, B, C, and D it can be proved that neither the foci nor the circular sections of the cones from the center of the

mirror and the portion where the light strikes normally will coincide except for $\theta = 0$; that is, for a mirror which is limited to the infinitesimal portion of surface at the vertex of the paraboloid.

(3) When the light making an angle C with the axis falls on a part of that principal section whose normal makes the angle θ with the axis.

$$\left[\frac{\cos^3\theta}{2a}\cos(\theta - C) + \frac{\cos^2(\theta - C)}{2s_0}\right]\left[\frac{\cos\theta}{2a}\cos(\theta - C) + \frac{1}{2s_0}\right] = 0 \quad (44)$$

$$s_0 = \frac{a}{\cos\theta\cos(\theta - C)}, \quad s_0 = \frac{a\cos(\theta - C)}{\cos^3\theta}.$$

The position of the circular image requires approximately that

$$s_0 = \frac{a\left[\cos^2\theta + \cos^2(\theta - C)\right]}{2\cos^3\theta\cos(\theta - C)}.$$

Taking the z's and x's of these points we find that for the foci or the circular image to coincide with those given by the portion of the paraboloid immediately around the vertex it is necessary that a relation should exist between C and θ, which shows that, in general, the light oblique to the axis and falling on any point of the mirror will not be brought to focus where the light which falls on the center part is condensed, nor will the circular images coincide. The interpretation of this result is found in this statement: That there does not exist any field, plane or curved, on which the image of the star off the axis is either a sharp diffraction disk (either round or linear) or a single blurred circle.[1] There seems, then, no way by use of curved plates to avoid the fan-shaped images[2] with an instrument having a large angular aperture. Very near the axis, of course, the distortion is negligible.

YERKES OBSERVATORY,
June 1900

[1] MCLAREN, "On the images formed by reflecting mirrors and their aberration," *M. N.*, **47**, 1886–7, p. 404, note.

[2] TENNANT, place cited.

REVIEWS

A GENERAL CATALOGUE OF 1290 DOUBLE STARS DISCOVERED FROM 1871 TO 1899 BY S. W. BURNHAM.[1]

ASTRONOMERS have known for a number of years that Mr. Burnham has been engaged in collecting measures and other material for the formation of a general catalogue of the double stars. Such a catalogue is indispensable to all double star observers and at present none exists in print.

This work in manuscript form has been patiently kept abreast of the times and has been in a condition for publication for many years, but its appearance has been prevented by a lack of means to print it. It is the only general catalogue of double stars in existence — except that of Innes', which deals alone with the southern stars. The work is a very large and complete one, consisting of twelve manuscript volumes covering the measures and history of every known double star from the north pole to 31° of south declination.

That Mr. Burnham has expended his best energies for a quarter of a century in its compilation is a complete guarantee of its very high value. It has not yet been possible to get the money to print this most important work and it still lies in manuscript form in his office, where double star observers throughout the world are constantly forced to apply for information on the subject of double stars.

Perhaps it was not known to many that Mr. Burnham had also another special general catalogue of double stars, always kept up to date in manuscript, but of less extent than the greater catalogue, for it contained only his own discoveries of double stars which had been made by him at various times and with a most varied assortment of telescopes. This most complete history of nearly 1300 double stars, many of which are among the most interesting and important yet discovered, after lying for many years in his office in Chicago, has at last assumed the form of a very handsome quarto volume admirably printed by the University of Chicago Press.

[1] *Publications of the Yerkes Observatory of the University of Chicago*, Volume I, University of Chicago Press, 1900.

Perhaps no other gift of the late Miss Catherine W. Bruce was of more direct benefit to astronomy than the publication of this volume, for it is to her generosity that astronomers are indebted for the printing of this splendid work. The volume is No. I of the *Publications of the Yerkes Observatory* and consists of 288 pages, with an appendix of six pages more containing the latest observations of Burnham's stars, which were received too late for insertion in the body of the work. The title is: *A General Catalogue of 1290 Double Stars, Discovered from 1871 to 1899, by S W. Burnham.*

The work is handsomely illustrated with half tone cuts of the Lick, the Yerkes, and the Dearborn telescopes, not omitting the famous 6-inch, now at the University of Wisconsin. A beautiful half tone cut of the Yerkes Observatory forms the frontispiece and numerous diagrams illustrate the orbits of the more interesting of the double stars.

To one who has so often heard Mr. Burnham affectionately speak of Baron Dembowski, that most skillful observer, the dedication of the present volume is almost pathetic in its language, for Dembowski was the first one to recognize and encourage the struggling amateur:

TO THE MEMORY OF

BARON DEMBOWSKI

THE DISTINGUISHED DOUBLE STAR OBSERVER, THE FIRST TO UNDERTAKE THE SYSTEMATIC MEASUREMENT OF THESE STARS, AND WHOSE KINDLY CRITICISM AND GENIAL ENTHUSIASM WERE TO THE WRITER ALWAYS AN INSPIRATION

THIS VOLUME

IS GRATEFULLY INSCRIBED

That Mr. Burnham's gratitude has kept fresh through all the years is further shown in his remarks in the introduction, which as they touch upon an observer who is so little known generally, except to double star observers, are worthy of transcription here.

I was fortunate in being placed in communication with this eminent astronomer soon after the commencement of my work with the 6-inch refractor, and from that time on until his death in 1881, I was in constant correspondence with him, and all of my discoveries were transmitted to him in advance of their publication. These new stars were measured by him in the most painstaking and thorough manner, and his observations give the fundamental data for comparison with subsequent measures of many of the most important of these new systems. As an observer with the micrometer he had no superior, and few if any equals. His work is of the highest degree of accuracy. He made no mistakes and wasted no time in idle speculations.

He has left a record of honest, thorough, and consistent work, which will be an honor to his memory for all time. Baron Dembowski was to me an example so inspiring, a critic so genial and frank, a friend so warm hearted and disinterested that simple justice as well as friendship impels me to inscribe this volume to his memory.

The introduction contains an admirable and concise historical account of Mr. Burnham's early work and is highly instructive.

After trying several small telescopes with object-glasses of foreign make, he finally, about 1869, arranged with Alvan Clark & Sons for a 6-inch refractor, stipulating only that it should be the best they could make.

This telescope soon became famous from the discovery of difficult double stars, and doubtless did more to give the Clarks their high prestige than any other instrument they ever turned out, excellent though they always were.

With this instrument Mr. Burnham discovered 451 new double stars, many of which were difficult objects in far greater telescopes. These discoveries were made after the sky had been swept over by the best double-star observers with much more powerful telescopes. This fact, however, Mr. Burnham modestly attributes to the superior defining power of his telescope. Though doubtless many of these stars depended on that very defining power for their detection, it is hardly necessary to state that it was mainly due to the unexcelled skill of the observer himself that they were found. Without the man — the right man — the best instrument is useless. Indeed, in this very connection there is nothing more instructive in the literature of astronomy than the account, quoted by Mr. Burnham in the volume, of Mitchel's visit to Sir James South in 1842, when seeking information for a large refractor for the Cincinnati Observatory. Mitchel's own account of his visit runs thus:

> One apartment was examined after another, until finally we reached a large room surmounted by a dome of great size and of an expensive construction, while fragments of the framework for mounting a great equatorial were scattered around.
>
> "Here, sir," exclaimed Sir James, "you behold the wreck of all my hopes. Here I have expended thousands, and flattered myself that I was soon to possess the finest instrument in Europe, but it is all over, and there's an end."
>
> I remarked that the object-glass was still in his possession, and might yet be mounted, so as to realize his hopes and expectations.

"No," said Sir James. "Struve has reaped the golden harvest among the double stars, and there is little now for me to hope or expect."

It would be difficult to appreciate the feelings which at that moment were sweeping through the mind of the astronomer. Long-cherished visions of fame and high distinction, nay, perhaps of grand discoveries in the heavens, which for years had played around his hopes of the future, had fled forever. Another had reaped the golden harvest, and like Clairaut, who wept that there was not for him, as for Newton, the problem of the universe to solve, Sir James South could almost weep to think that another's eye had been permitted to sweep over the far distant realms of space which he had long hoped might remain his own peculiar province.

The instrument referred to was a 12-inch refractor.

Perhaps it is not out of the way to add here that Sir James South was so disappointed with the delay in the finishing of his mounting, the unsatisfactory condition in which it was finally furnished him, and the consequent loss of the chance for discovery that, as if to more fully accentuate these lost chances, it is said, he finally hired a healthy and vigorous blacksmith and a heavy sledge hammer to smash up and utterly destroy all of the telescope with the exception of the object-glass itself, which he finally gave away. This was over a quarter of a century before the advent of the celebrated 6-inch!

Mr. Burnham's high opinion of the Clark object-glasses is frequently shown in the volume as, for instance, in the case of *85 Pegasi* (p. 269): "It is an excellent test for the definition of any object-glass, however large, and no instrument, whatever its aperture, can deal with a pair of this class unless the figure of the objective will compare favorably with the Alvan Clark standard." And this is very true indeed.

Mr. Burnham is very cautious and makes few or no mistakes. A beautiful confirmation of his accuracy has but just occurred, at a time too when he himself was somewhat shaken in his own faith. In 1871 he found with the 6-inch that the companion to Rigel was a very close double (pp. 59, 60); the star appeared elongated in the small instrument. He subsequently saw and measured it with the 18½-inch at Chicago, in 1878. From 1879 to 1898, with the single exception of an observation by the Henry Brothers at Paris, in 1884, the star was apparently single in all instruments; even the 36-inch at Mount Hamilton failed to show it, though it was under observation for four or five years. The repeated failures to even elongate the star with the most powerful instruments gave the impression that some mistake must have been made in the early observations with the smaller instruments. In

the very passing of the proofs of this catalogue through the press, Mr. Burnham had written (p. 60). "One of two conclusions seems obvious, either this star is not double at all, the elongation supposed to be seen on the different occasions mentioned being due to atmospheric or other causes; or, if double, it must be moving with great rapidity. The negative results can be explained in no other way. I dislike to believe that I have been deceived by any spurious elongation of the small star, as this would be the first time such a mistake has happened in my experience in double-star observations; but certainly my subsequent failures to see this star double would tend to that conclusion."

It remained thus until November of 1898, when Aitken, who had watched it at Mr. Burnham's request with the 36-inch, again found it to be double, but excessively close and difficult. It has since been seen with the 40-inch of the Yerkes Observatory. Mr. Burnham thinks that possibly this star may have a shorter period than that of any known binary.

Of late years Mr. Burnham has frequently shown the absurdity of many of the computed double-star orbits. In several cases he has shown that a straight line would as faithfully represent the observations as any of the orbits that had been computed from them. As an illustration of his keen insight into the problems of double-star motion, we may take the case of *9 Argus*, β 101 (pp. 92, 93, 94). This star was known to be a binary, and an orbit had been computed for it with a period of over forty years. In 1892 Mr. Burnham, by a judicious selection of observations, computed a new orbit with a period of 23.3 years. This differed entirely from the previous one in all the elements. So strong was his faith in the correctness of the orbit that he unhesitatingly predicted that in the two years following his measures of 1892, the star would move through more than 180° of its orbit, a prediction which was fully confirmed by the great rapidity of the star's motion at that time.

It is not only his good judgment, but also his knowledge of the fallibility of many observers that enables him to discriminate and reject unreliable observations, and thus avoid the pitfalls that come to the average computer, who must take all observations on faith and construct an orbit not only to fit the accurate measures, but to fall in with the poor ones as well. In work of this kind there is nothing like knowing the proper value to put on the available material, and especially in knowing where to prune out without mercy, and this characteristic seems to be highly developed in Mr. Burnham.

His remarks about telescopes are always good, and a comparison of his 6-inch with some of the larger instruments, that preceded it in double star work, must be interesting. In this particular he says:

But it must be remembered that at least some of the instruments used by these observers could not compare favorably with modern refractors, and particularly with telescopes made by the Clarks; and even when these observers had more powerful instruments in point of light-grasping power, as in the case of the Herschels, there can be no doubt that they were far inferior in definition, and in every practical respect for observations of this kind, to the 6-inch refractor. . . . . A glance at the list of old pairs to which new and more difficult components have been added will be sufficient on this point. I have shown in the appendix to my thirteenth catalogue that my several lists which had been published at that time included more double stars of Class I (where the distance does not exceed 1 ″) than all the various catalogues of both Herschels and both Struves, notwithstanding the fact that the works of these eminent astronomers contain altogether not less than 7400 double stars.

The arrangement of the catalogue is an admirable one and should serve as a model for all such work. The salient facts concerning each star are given in the briefest yet most comprehensive form. An excellent index to the different stars is found at the close of the volume, from which any one of the objects can at once be found in the body of the work.

Mr. Burnham pays a just tribute to Aitken of the Lick Observatory, who has furnished him with a very large number of measures of these stars up to the moment of issuing the volume, and which are of high importance, giving, as they do, the very latest information of many of the stars.

In the catalogue there are 18 quadruple stars of the ϵ *Lyrae* type, but while the distance between ϵ^1 and ϵ^2 *Lyrae* is about 207″, the greatest distance between any of the pairs in this catalogue does not exceed half that amount, and the closest of these pairs is not separated by more than 11″.

The catalogue contains a large percentage of the shortest period binaries, including the celebrated κ *Pegasi*, whose period, 11.37 years, is the shortest known.

The observations show that there are no less than 185 β stars which are binaries, and doubtless many times that number will be shown to be physically connected pairs when sufficient time has elapsed. Of

these, 27 pairs certainly have periods of less than 50 years and 5 have periods ranging between 15 and 30 years, such as

β 101,	*9 Argus*	23.3 years
β 733,	*85 Pegasi*	25.7 years
β 151,	*β Delphini*	26.7 years

whose orbits were computed by Mr. Burnham. He shows that β 883 does not have the short period of 5½ years assigned it by one computer. He further shows that at present the data are not sufficient to definitely determine the period of this star, though he concludes that Glasenapp's period of 16.35 years, determined from observations up to 1891, is near the truth.

Perhaps one thing as striking as any other in the volume is the evident care each one of the stars receives. The proper motion is given in every case where possible. All the measures of the different components, if the star happens to be a new member of an old system, are given. Mr. Burnham has exhausted every known source for proper motion, and in many instances has been able to supply this from his own measures. The proper motions are very valuable, for though no relative motion should be shown in a double star, it yet may be known to be a binary by the common proper motion of its components. Several hundred cases of proper motion are given. As so many of these stars are not only of the highest interest, but are also extremely difficult objects, they have engaged the attention of the best observers in this class of work with the finest instruments yet made. This volume, therefore, containing the observations of these stars, has an additional interest from this point of view alone.

The history of each star is briefly given in tabular form, including the measures of the different components, if it is a multiple star. Outside of the actual importance of these measures for orbit investigation, it is a most interesting summary, as it shows side by side the work of the different observers; those who have passed away are linked to the present generation by their work on these stars. Take, for example, the quadruple system of *ν Scorpii* (pp. 148, 149). There seems to be no certain evidence of change in any of the components of this star since their discovery. For the star *C*, measures are given from 1782, of *D*, from 1846, and of *A* (β 120), from 1874. The first of these stars has been measured by six observers, including William Herschel; the second by thirteen, and the third by fifteen observers. In all,

twenty individual observers have measured one part or the other of this multiple star in the past one hundred and fifteen or twenty years. Each one has had his try at it and his measures remain to us as an evidence of his skill. They all show, however, the remarkable fixity of the system of *ν Scorpii*, which remains immutable while the observers come and go. Comparing this star with *9 Argus* about 1892 to 1894 we have essentially the extremes of double star movements.

Mr. Burnham points out a remarkable case of proper motion in connection with *Aldebaran* (pp. 49, 50, 51). A new companion to this star was found by him in 1877 with the 18½-inch at Chicago. The measures show that this small star has the same proper motion as *Aldebaran*, and it is therefore a physical companion ; while the old 11th magnitude companion discovered by Herschel over a hundred years ago, and which Mr. Burnham found to be double in 1888, has a proper motion apparently independent of *Aldebaran*, and hence seems to be an independent system of its own seen in the direction of *Aldebaran*. Such proper motion in so small a star is remarkable, and it would almost seem, in spite of the evidence given, that it is also a physical companion to the large star, and that the difference of proper motion is really due to orbital motion of the smaller star.

When it is known that the great number of measures in this volume made by Mr. Burnham himself is not the largest part of the great mass of measures of double stars he has made during a very busy life, having in general no connection with astronomy whatever, it will be seen that he has wasted none of the time at his disposal for telescopic work. To accomplish all this he has had to work much faster than the average observer. His measures are made very rapidly, so that he usually spends only a few minutes with a star. The time is further abbreviated by his singular method of recording. He always makes three settings of the wires before recording them in his notebook, mentally carrying each successive reading until the three are made, and these are then written down. His notes are sometimes put down in stenographic characters, for he was once an expert stenographer.

Perhaps no other observer has used such a varied assortment of telescopes. Beginning with the 6-inch, or still earlier, with a 3-inch, he has not only used for measuring, but has discovered double stars with the 6-inch, 9.4-inch, 12-inch, 15½-inch, 16-inch, 18½-inch,

26-inch, 36-inch, and finally with the great 40-inch of the Yerkes Observatory.

This is shown by the following tabulation of the number of double stars discovered with each instrument (p. xi).

With the

6 -inch	Private Observatory	451
18½-inch	Dearborn Observatory	413
36 -inch	Lick Observatory	198
15½-inch	Washburn Observatory	87
9.4 -inch	Dartmouth College Observatory	24
26 -inch	Naval Observatory	14
40 -inch	Yerkes Observatory	8
16 -inch	Warner Observatory	2

Among these powerful instruments it is pleasing to see that the greatest number of discoveries with any one telescope was made with the 6-inch. One somehow takes far greater pride in this, for it represents the labor of a struggling amateur, who during the day led the drudging life of a stenographer in the United States court in Chicago, and at night worked among the stars for the pure love of it. Such work deserves an everlasting fame, and surely this has fallen to Mr. Burnham.

E. E. BARNARD.

Photometric Revision of the Harvard Photometry during the Years 1891–1894. By EDWARD C. PICKERING. *Annals of the Astronomical Observatory of Harvard College*, Vol. XLIV, Part I. Cambridge, 1899.

THE measures forming the original *Harvard Photometry* were made during the years 1879–1882 with the small meridian photometer. When the large meridian photometer, aperture 10.5 cm, was returned from Peru, a revision of these measures was undertaken, and the present important volume gives the result of this revision, together with measures of comparison stars for variables and miscellaneous objects, about 7000 in all.

On examining this work one is struck with the condensed form in which the results are given. Several columns found in the *Harvard Photometry* (*HP*) are omitted; the first four columns give, successively, the *DM* number of the star, the right ascension and declination for

1900, and the *DM* magnitude. As the *HP* number is not given, and the epoch is twenty years later than for the star-places in the *HP*, reference to the latter work is rendered less convenient, but the saving in space is quite justified. The next three columns give in succession the mean photometric magnitude, the Julian day of each observation, and the corresponding residual, found by subtracting the mean from the several results. In this condensed form, the date and magnitude from each day's measures can be found, a very important point, in case variability is later suspected.

The number of observations of each star is generally three, though in many cases it is considerably increased, sometimes reaching forty. For the comparison stars for variables the number is frequently two.

A complete discussion of the results in this volume, giving comparisons with other photometric catalogues, would be of great interest, and will doubtless appear in a later volume of the Harvard *Annals*. In the meantime it seemed worth while to the writer to make a partial comparison with the *Harvard Photometry* (*HP*) and the *Potsdam Photometric Durchmusterung* (*PDM*). For this purpose every twenty-fifth star in the *HP* was taken, numbers 1, 26, 51, 76, etc. It was found that 165 of these stars were contained in the volume under discussion, which will be called the *Revision* (*Rev*), and of these eighty-five fell within the limits of the *PDM*. On comparing the *Revision* with the *Harvard Photometry*, the mean difference in the sense *Rev—HP* was found to be $+0^m.012$, showing that the systems in the two volumes are practically identical. The mean difference, without regard to sign, was $\pm 0^m.131$. The distribution of the differences is shown in the following table:

Range in $0^m.01$	Number of residuals
0 to 4	38
5 " 9	37
10 " 14	35
15 " 19	19
20 " 24	14
25 " 29	7
30 " 34	5
35 " 39	4
40 " 44	5
> 44	1

The greatest difference was for *HP* No. 2776 $-0^m.59$. This star has four observations in the *HP* and five in the *Rev*.

The comparison with the *Potsdam Photometric Durchmusterung* (*PDM*) gave similar results. Of the sixty-six stars common to each the mean difference was found to be, $HP-PDM = -0^m.206$; $Rev-PDM = -0^m.189$. Although this depends on a relatively small number of stars, it is shown to be fairly representative by the fact that in Part II of the *PDM*, page 456, all the stars common to it and the *HP* gave difference $HP-PDM = -0^m.18$. Adding the constant $0^m.18$ to the differences, the means became as follows:

$$HP-PDM = -0^m.024\,;\quad Rev-PDM = -0^m.008$$

The differences without regard to sign then became:

$$HP-PDM = \pm 0^m.18\,;\quad Rev-PDM = \pm 0^m.16$$

A considerable part of these latter residuals is doubtless due to systematic differences in the measurement of stars of different colors, as is shown by Müller and Kempf in *PDM*, II, 459 (see also ASTROPHYSICAL JOURNAL, **10**, 70).

The only typographical error noticed is the declination of ϵ *Eridani* (*HP* 576), which is given as north in the *Revision*. J. A. P.

ERRATA.

ASTROPHYSICAL JOURNAL, Vol. XII, No. 1. In title of Plate VIII, *for* 6½ feet Focal Length *read* 61½ feet Focal Length.

Vol. XII, No. 2, p. 163, line 25. *For* enlarged 22 diameters *read* enlarged 2.2 diameters.